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THE WEDGE THEORY OF DIASTROPHISM

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ABSTRACT

Field studies on the depth of folding, laboratory experiments in faulting with analysis of stress-strain relations, and the two-sided character of various mountain systems, have led to the conclusion that the wedge-shaped block is the typical form of compressed mountain ranges. The typical wedge apexes downward in the middle of the deformed zone. Other considerations have resulted in the extension of the wedge shape and wedge dynamics to plateaus and to the elevation and deformation of continents.

This paper outlines the development of the theory, after which the folding of mountain chains, the elevation of plateaus and the deformation of continents are discussed briefly in terms of the wedge principle. Consideration is also given to the means of adjustment along the wedge borders, to the reasons for the asymmetry of mountain wedges, the importance of relative position on the continents and in the wedges in determining the direction of overfolding, and to certain aspects of the related vulcanism.

INTRODUCTION

In 1905 field measurements were made across a strip of the Pennsylvania Appalachians from Tyrone to Harrisburg, to determine the amount of crustal shortening and other changes involved in the folding and faulting of this portion of the Appalachian Mountains. As a result of calculations based on the horizontal shortening and the resulting vertical bulge, the configuration of the under surface of the deformed mass was obtained. The shape of the deformed mountain mass was found to be that of a triangular prism or wedge,

apexing downward beneath the middle portion of the folded belt.[1] While this wedge-like shape was a development quite unexpected, it did harmonize so admirably with theoretical considerations that further extension of these wedge principles to other mountain ranges, to the uplift of plateaus, and to the elevation of continents, seemed the natural outcome.[2] While many data have been accumulated for the further application and development of wedge dynamics, and various mountain systems found which bear out these principles, the present writer has published little on wedge dynamics, but contented himself largely with preaching the doctrine to graduate students. It seemed advisable to mull over the ins and outs of the problem more thoroughly.

Now there have recently come to attention two major works by Kober, the outcome of long continued and carefully considered researches, which urge the two-sided symmetrical character of mountain ranges, and which picture a somewhat similar wedge-shaped deformed mass between two much less deformed forelands.[3] Lateral thrusts through the bordering forelands are believed to cause the squeezing of the weaker materials of the intervening trough into mountain ranges, which develop most strongly near the margins of the deformed zone, as two sets of bordering chains, called Randketten. The middle or less deformed portion of the squeezed geosyncline between the marginal Randketten, Kober has termed the Zwischengebirge.

Kober's bold synthesis places the mountain systems of the general Mediterranean region in two Randketten: (1) the Alpides, comprising the northern marginal chain of Pyrenees, Alps, Carpathians, Balkan Ranges, Caucasus, etc.; and (2) the Dinarides, or southern marginal chain, composed of the Atlas, Apennines, Dalmatian, and Greek systems. Between these two great chains less deformed tracts appear in the western Mediterranean, Hungary, Thrace, the Aegean,

[1] Rollin T. Chamberlin, "The Appalachian Folds of Central Pennsylvania," *Jour. Geol.*, Vol. XVIII (1910), pp. 228–51.

[2] Rollin T. Chamberlin, "The Building of the Colorado Rockies," *Jour. Geol.*, Vol. XXVII (1919), pp. 145–64 and 225–51; "Megadiastrophism," Pt. II, *ibid.*, Vol. XXIX (1921), pp. 416–25.

[3] Leopold Kober, *Der Bau der Erde*, Gebrüder Borntraeger, Berlin, 1921; *Bau und Entstehung der Alpen*, Berlin, 1923.

and central Asia Minor. The northern, or Alpine chain, is characterized by structures overfolded toward the north, while the southern, or Dinaric system, shows structures overfolded toward the south. Throughout most of the general Mediterranean region these two opposing chains of mountains are wide apart, and their trends follow tortuous arcs which do not show very close relationship to one another. But in the region of the Alps proper, it is to be noted that both the Alpine and Dinaric chains come together in a very narrow strip with no intermediate zone. This seems to be a matter of prime importance and may, perhaps, furnish a key to some of the perplexing phenomena of the Alps.

Kober has sketched very appealingly the North American Cordilleran orogeny as a two-sided deformed mass between the Great Plains and the Pacific, which serve as forelands. The Rocky Mountains on the east and the Pacific Coast Ranges on the west, according to him, constitute the Randketten. The less deformed Interior Plateau between these border chains is the region of the Zwischengebirge.

But it is to be noted that the deepest portion of the deformed mass, according to Kober's cross-section diagram,[1] extends down many hundreds of miles beneath the surface. Likewise if we examine his interpretation of the great mid-Tertiary Alpine-Himalayan orogeny, we find that the two folded systems swing widely apart around the western Mediterranean, also in Asia Minor, and again in the Himalayas and the Kuen Lun ranges with Tibet between them. Since 700 to 800 miles separate the outermost Kuen Lun and Himalayas, a wedge of which they are the border chains would necessarily extend to great depths beneath Tibet. Deep orogenic roots in general characterize Kober's diagrams, though he has not given a diagram grouping together the Kuen Lun and Himalayan ranges. The farther apart the border chains, obviously the deeper the deformed zone must be, if one projects the bordering faults, or border shear planes, at uniform angles under the mass.

This possible difficulty of great depth confronted the writer in an attempt to apply the Appalachian wedge to the Kuen Lun-Himalayan combination in 1910. Using at that time the 45° principle for

[1] *Bau und Entstehung der Alpen*, p. 4.

the bounding shear planes, a Kuen Lun-Himalayan block was seen to require a depth of more than 300 miles under central Tibet. The important relationship between horizontal shortening, depth of deformed mass, and consequent vertical bulge of the mass, did not make such a depth of folding beneath Tibet seem altogether consistent with the probable amount of folding in the plateau area.

Later, when field work in Alaska in 1919 brought vividly to the writer's attention the fact that the cleavage and schistosity of the metamorphic rocks all along the coast of British Columbia and southern Alaska was inclined prevailingly northeastward and northward in under the curving Pacific ranges, while the Rockies on the the other side of the Cordilleran mountain province were known to be overturned and overthrust in the opposite direction, exactly the same problem came up. Wherever the steamers stop along this coast —at Prince Rupert, Ketchikan, Juneau, Cordova, Valdez, etc.—the schistosity of the strongly deformed rocks dips in under the Coast Ranges, the Chugach Range, etc., at angles hovering steadily around 50°–55°. Can this be the other side of a great wedge of which the well-known Rockies constitute the eastern border chain? Were these coastal ranges formed simultaneously with the Rockies on the east? Would the depth of the wedge-roots as naturally drawn beneath the Interior Plateau to be too great to allow the whole Canadian Cordillera to be taken together as a single wedge?

An answer to the last and similar questions of depth of diastrophism is suggested by the well-known views of T. C. Chamberlin on internal reorganization, diastrophism and vulcanism,[1] but the present writer has wished to confine himself as strictly as possible to inductions from observational data. He has been conservative in extending the wedges to great depths. At the same time, the full bearings of these studies cannot be appreciated without some recognition of their historical relations to the studies of T. C. Chamberlin from the cosmologic point of view.

INDUCTIVE DEVELOPMENT OF THE WEDGE THEORY

Field studies.—As already stated, the deformed mass representing the Appalachian Mountains west of Harrisburg, Pennsylvania,

[1] T. C. Chamberlin, "Disatrophism and the Formative Processes," *Jour. Geol.*, Vols. XXII, XXIII, XXVI, XXVIII, XXIX (1913–21), Articles I–XV; also *The Origin of the Earth* (1916), chaps. viii and ix, pp. 150–240.

was found to be a triangular prism with two sides converging beneath the mountainous tract till they come together under the middle portion of the deformed belt at a depth of 32 miles.[1] No consideration whatever of stress-strain relations entered into the deduction of the wedge-shaped form. It came directly from calculations and graphic treatment of the field measurements, quite irrespective of any theory of mechanics, or theory of diastrophism. But it was soon seen that this shape was in harmony with the principles of fracture-deformation under horizontal compression, and that the principles developed were likely to be of very general application. The wedge-shaped block and wedge dynamics in deformation seemed to open up possibilities along a new line. In view of this, it seemed desirable to apply this method of inquiry to some other mountain systems. For the next trial, the Rocky Mountain system was the most available.

The Rocky Mountain system, as is well known, is bordered on its eastern margin through many degrees of latitude by great overthrust faults. In these the overriding sheets of Paleozoic and pre-Cambrian rocks have come from the west and have moved eastward over the younger strata of the Great Plains. In Alberta and Montana, where the overthrusting has been strongest, there are, in addition to the bordering overthrusts, many lesser thrusts, of the slice-fault variety. These fault planes dip westward in under the mountains. This arrangement is the typical border arrangement called for by the wedge theory. But it is only one border—the eastern border. Is there a corresponding western border?

In the summer of 1910 the writer went into the field with the belief that the west front of the Mission Range in Montana and the east side of the Rocky Mountain Trench in southern British Columbia were among the most likely places to find easterly-dipping thrust faults, and other related structures, which would constitute the western counterpart of the overthrust eastern Rocky Mountain border. In this, however, owing to lack of time and the difficulties of the country, the writer was unsuccessful. More recently Roy A. Wilson has found the suspected easterly-dipping thrust fault and easterly dipping shearing planes at the west base of the Mission Range near Flathead Lake,[2] and F. P. Shepard has found the related

[1] *Jour. Geol.*, Vol. XIII (1910), pp. 228–51. [2] Roy A. Wilson, unpublished thesis.

structures along the Rocky Mountain Trench. He has described the faulting as dominantly of the thrust type, the shear planes, the axial planes of the folds, and the slaty cleavage on the west side of the trench mostly dipping to the west, while the structural planes on the east side of the trench dip persistently to the east under the Rocky Mountains.[1]

The Rocky Mountains of Alberta and northern Montana, limited on the west by the Rocky Mountain Trench, are thus characterized on their western border, as on their eastern margin, by thrust planes and shearing zones which dip inward under the mountain range. This makes the wedge. It is, however, not a symmetrical wedge, for the overthrusting on the east border is much stronger than that on the west border. This is, perhaps, to be expected, owing to the position of the Rockies along the eastern side of the broad strip of Cordilleran deformation. Quite recently R. F. Flint has reviewed the literature to show that somewhat analogous wedge-like structural conditions exist in various other sections of the Rocky Mountain system.[2]

EXPERIMENTAL SUPPORT OF THE THEORY

In 1917 an experimental investigation of continental deformation was undertaken with J. T. Richards, but never completed. A machine for compressing blocks of the shape of a continental sector of the earth was built with movable steel jaws hinged below and corresponding in position to radii of the sphere.

Into this a triangular mass was molded with a surface curvature similar to that of a continent. By use of a powerful jack, this representative of a continent could be squeezed by varying pressures reaching up to many hundreds of pounds per square inch. For experiments upon materials of uniform strength, pure paraffine was used; for experiments embodying the principle of increasing strength of materials with increasing depth below the surface in accordance with Adam's deductions from his experimental researches, there were molded, one above another in a series of zones of predetermined resistances, mixtures of plaster and paraffine in definite proportions. In all these experiments the prev-

[1] Francis Parker Shepard, "The Structural Relation of the Purcell Range and the Rocky Mountains of Canada," *Jour. Geol.*, Vol. XXX (1922), pp. 89–99.

[2] Richard Foster Flint, "A Brief View of Rocky Mountain Structure," *ibid.*, Vol. XXXII (1924), pp. 410–31.

alent type of faulting was that of a triangular wedge (see Figs. 1 and 2). . . . . This wedge-faulting naturally enough appeared first in the surficial portion of the sector; later, after some relief had been realized above, and movement had become less restrained, additional fracturing set in below.[1]

FIG. 1.—Continental sector compressed. Upper white zone, 90 per cent paraffine, 10 per cent plaster; upper black zone, 80 per cent paraffine, 20 per cent plaster; middle white zone, 70 per cent paraffine, 30 per cent plaster, etc. Pressure applied by jack-screw on left steel jaw. After compression the steel jaws have fallen back to their original positions, thus showing the amount of compression.

[1] R. T. Chamberlin and J. T. Richards, "Preliminary Report on Experiments Relating to Continental Deformation," *Science*, Vol. XXXXVII (1918), p. 492.

These experiments involved, of course, short-time stresses, for long-time stresses cannot well be handled experimentally. It is not necessary for a continental sector to extend to the center of the earth

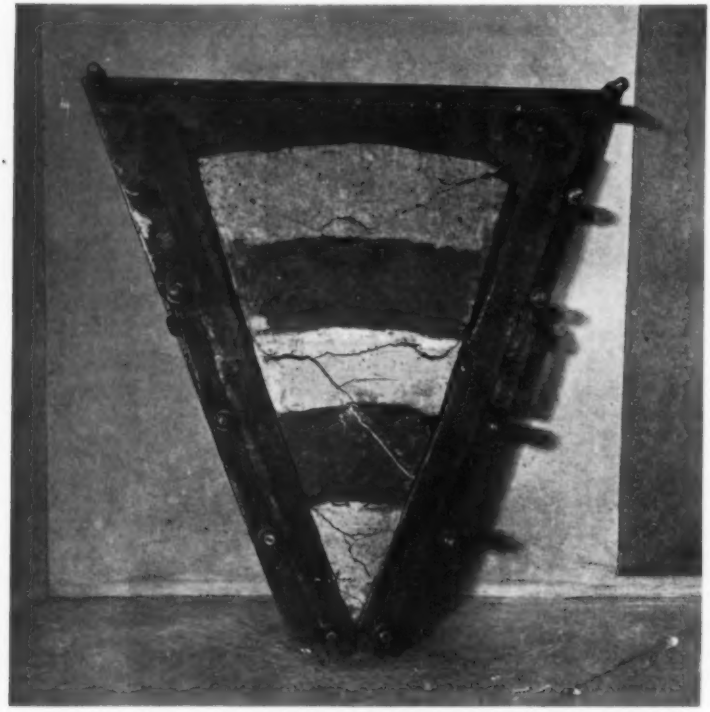

FIG. 2.—Materials same as in Figure 1. The wedge fracture in the top layer formed first. With further compression fracturing set in lower down. Note how the heavy 2×4 steel jaw on the left has bent under the compression. The shortening above has been much greater than that below.

to produce these results. The writer ventures no opinion as to the depth to which continental sectors extend.

Sand wedges.—If a small pile of loose sand be leveled off above, and two blocks of wood be pushed forward simultaneously into the sand from opposite sides, wedge faulting results, and the

wedge of sand between the fault planes is thrust upward. Figures 3, 4, 5, and 6 show specimens of wedge faulting in loose sand. Using the method devised by Mead,[1] flowers of sulphur were mixed with the sand and, after faulting, the whole apparatus (sand and blocks undisturbed) was heated in an oven at 140°C. until the sulphur melted and bound the sand grains together. Upon cooling an artificial sandstone was obtained. In these tests the inner wedge formed first, and after it had risen considerably above the level of the sand surface on each side, the outer set of faults developed. This is also the order of succession found in mountain building; faulting of this sort progresses outward into the undeformed material adjacent. In this way the wedges become more and more complex.

[1] Warren J. Mead, "The Geologic Rôle of Dilatancy," *Jour. Geol.*, Vol. XXXIII (1925), p. 688.

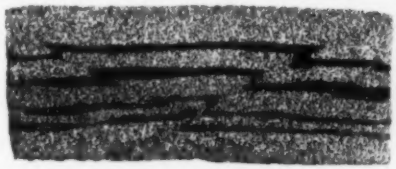

FIG. 3.—Wedge faulting in loose sand. Pressure applied simultaneously from both sides. On extreme right another fault is just beginning outside the original wedge.

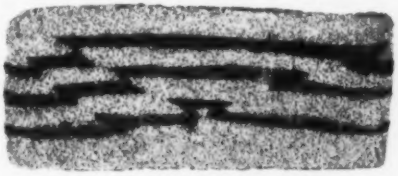

FIG. 4.—Wedge faulting in loose sand. A second fault forming on left. In this way the wedge is enlarged as the deformation and shortening progress.

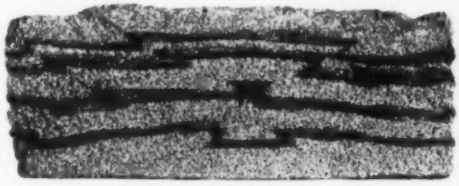

FIG. 5.—Wedge faulting in loose sand. In this case a wedge pointing downward also developed.

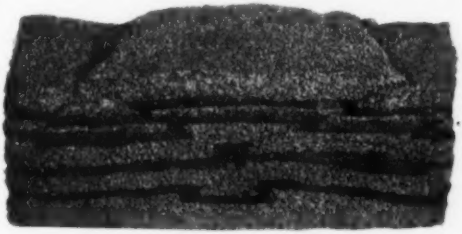

FIG. 6.—Other half of sawed sandstone block used for Figure 5. To show the surface bulging resulting from the wedge action.

APPARENT EXEMPLIFICATIONS OF THE THEORY

I. MOUNTAIN WEDGES

The value of any theory lies largely in its working qualities. Let us now examine some of the representative folded mountain systems of the globe in the light of the wedge theory to determine, from the data available at the present time, whether or not they are in harmony with the theory.

Appalachian wedge.—Considering the entire breadth of the Appalachian orogenic system, comprising folded and faulted Paleozoic formations in the western portion, and more intensely deformed metamorphic rocks of less certain age in the eastern portion, it is seen that in the western part the folds are overturned toward the northwest. Likewise the planes of the thrust faults dip, with but few exceptions, to the southeast. On the other hand, in the eastern portion, where the structures are much less easily deciphered, Keith has recognized belts in which the axial planes of the folds and the planes of faulting dip northwestward.[1] From New Jersey south, the later Coastal Plain sedimentaries conceal much of the eastern side of the folded Appalachian belt. Along the Connecticut shores, again along the coast both north and south of Boston, and on the Maine coast, the schistosity and allied structures dip landward, for the most part at steep angles. The folded tract presumably extends somewhat beyond the shores, so that its outermost eastern border is not accessible. If it were accessible, one should expect to find there northwesterly-dipping structures inclined at somewhat lower angles. Woodworth has stated that the overturning of the structure toward the southeast extends along the Atlantic Coast from southern Maine to North Carolina.[2]

Because of the opposing dips on the two sides, a general fan-shaped arrangement of the structures characterizes typical cross-sections of the Appalachian folded belt when taken in its entirety. Lack of information in the metamorphic eastern half of the fan

[1] Arthur Keith, "Outlines of Appalachian Structure," *Bull. Geol. Soc. Amer.*, Vol. XXXIV (1923), pp. 319–20.

[2] J. B. Woodworth, "Cross-Section of the Appalachians in Southern New England," *Bull. Geol. Soc. Amer.*, Vol. XXXIV (1923), p. 258.

makes it impossible as yet to judge how symmetrical the fan development actually was. But in any case, the fan arrangement indicates either underthrusting of the folded belt from both sides, or overthrusting of the two wings of the folded mass outward from the middle. This is the wedge principle.

The Appalachian system in toto is therefore believed to constitute a wedge mass whose eastern wing, being nearest the critical continental border zone, has suffered much greater deformation and metamorphism than the western wing. The original wedge, deduced from the folds of central Pennsylvania, which includes only the Paleozoic area west of the crystalline metamorphic belt, is only a part of the whole Appalachian wedge mass. Keith's descriptions, if they are correctly interpreted by the writer, seem to suggest also composite fan structures in the metamorphic belt. Further study, with increasing knowledge, will probably show that the Appalachian wedge is composite, comprising several smaller units in which the same general principles have operated.

Rocky Mountain wedge.—Studies in the Rocky Mountains have revealed both similarities and differences. The Lyons–Grand River section in Colorado presented difficulties owing to the impossibility of determining the amount of folding and crustal shortening over the pre-Cambrian granite cores in some of the upfolds. Nevertheless this section, in spite of its imperfections, indicates that the deformed zone was very shallow on the eastern edge of the Front Range where the Rockies rise from the Great Plains, and that, taken in the large, it deepened gradually toward the west, like the side of a wedge, till it merged into the deeper plateau type of deformation.[1] The Gore (Park) Range and the Front Range were individually distinctly wedge-shaped.

In southern Colorado the Rocky Mountain folded ranges converge till they come together near La Veta Pass, from which point southward into New Mexico the entire Rocky Mountain system comprises but a single mountain ridge, the Culebra Range.[2] The pat-

[1] "The Building of the Colorado Rockies," *Jour. Geol.*, Vol. XXVII (1919), pp. 242–47.

[2] *Ibid.*, pp. 148–50.

tern resembles the letter Y. Here is a capital illustration of the manner in which a mountain system dies out.[1] It tapers, becoming narrower and presumably shallower toward its extremity. This implies the wedge shape. The studies of other investigators substantiating the wedge-like character of the Rocky Mountains farther north, have already been referred to.

Caledonian wedge.—The Caledonian orogeny in western Europe at the close of the Silurian Period exhibits also the wedge-like form and dynamics. The western half is in Scotland; the eastern half in Scandinavia. In the northwest Highlands of Scotland, overthrusting toward the west-northwest has taken place along the famous Moine, Ben More, Glencoul, and other major thrust planes. Across the North Sea in Scandinavia, on the east side of the deformed belt, tremendous overthrusting toward the east-southeast has occurred along several great faults. In Scotland the fault planes, major and minor, dip eastward under the mountainous belt; in Scandinavia the corresponding fault planes dip westward beneath the disturbed belt. A downward projection of these opposing sets of shear planes would bring them together beneath the North Sea and beneath the shallow portion of the Atlantic on the continental shelf farther north.

Where visible today, the major thrust planes are characterized by low dips. But back from the overthrust margins of the orogenic wedge, it is probable that the deformed mass extends more deeply below the surface than would be indicated by projecting the bordering fault planes downward till they meet. Even so, however, the depth of the deformed wedge need not be much in excess of a hundred miles in its deepest middle portion.

North of 60° latitude, the overthrusting toward the east on the east margin in Scandinavia was particularly pronounced. To the south, in the British Isles, the overthrusting toward the west on the west margin was strongly dominant (Figure 7). This shifting of the greatest display of thrusting from one border of the deformed belt in one section to the other border in some other section, brings to the fore some of the special features of wedge dynamics. In either case the final result—whatever may have been the direction of the

[1] Rollin T. Chamberlin, "On the Crustal Shortening of the Colorado Rockies," *Amer. Jour. Sci.*, Vol. VI (1923), pp. 216–20.

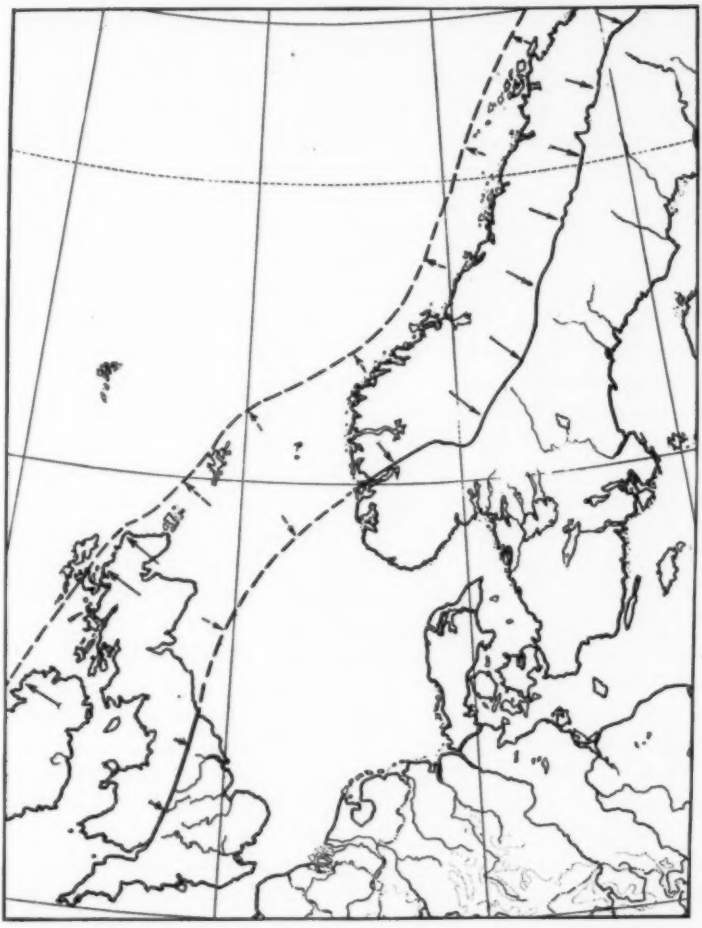

FIG. 7.—Map of a portion of the Caledonian orogenic wedge. In Scandinavia, where the compressed belt bends forward to the east, the overthrusting toward the east on the eastern flank has been particularly pronounced. In the British Isles, where the deformed belt makes a salient to the west, the overthrusting toward the west has been strongly dominant. Some pivot action, with the neutral point under the North Sea between the Shetland Islands and Norway, is suggested. The amount of overthrusting is represented by the relative lengths of the arrows.

active forces—is overthrusting outward from the middle of the wedge mass. The forces may operate as underthrusts from outside the wedge, but the margins of the wedge are overthrust outward. Since the Caledonian wedge shows greatest overthrusting toward the east on the east side in some sections and toward the west on the west side in other sections, it is apparent that local conditions must be important in determining whether the deformed mass is to give way mostly on one border, or mostly on the other. Wedges are not commonly symmetrical. One border or the other yields most.

A study of Scandinavia and Scotland alone—which, to be sure, are only a small part of the whole Caledonian chain—suggests the possibility of some sort of pivot action, which may be a factor contributing to the great amount of overthrusting. If some point under the North Sea to the northeast of Scotland served as a pivot about which the portion of the block to the north swung eastward, while the portion to the south swung westward, it would help explain the great extent of the horizontal overthrusting in opposite directions without necessitating so much general crustal shortening. If any such movement as this actually took place, it would presumably be accompanied by horizontal slippage, along essentially vertical faults, transverse to the axis of the Caledonian system. Transverse faults of this sort, or "flaws," are abundant in the Jura Mountains[1] and in the overthrusts of southern Nevada.[2] They seem to be a common means of adjustment between different portions of a folded and faulted range, where the degree of horizontal shifting varies from one section to another. In the northwest Highlands of Scotland transverse normal faults are abundant, but they dislocate the Caledonian thrust planes, and so have been interpreted as younger than the Caledonian orogeny.[3]

In the Scottish-Scandinavian block the wedge movement is taken to be the governing type of deformation. Any pivotal movement is subordinate to this, being in the nature of differential movement

[1] Albert Heim, *Geologie der Schweiz*, Vol. I (1919), Tafel XX.

[2] Chester R. Longwell, "Recent Faults and Flaws in Southern Nevada," *Bull. Geol. Soc. Amer.*, Vol. XXXV (1924), pp. 64–65.

[3] "Geology of the Northwest Highlands of Scotland," *Mem. Geol. Surv. of Great Britain* (1907), p. 477.

within the wedge, which might affect either the whole depth of the wedge, or only superficial portions of it.

Alps.—The wedge shape, though less symmetrically developed, appears in the Alps, the Pyrenees, and the Caucasus. Among the Alps the northern and central chains have been tremendously overfolded and overthrust toward the north, while in Italy, south of the main Mont Blanc-Pennine axis, the structural axes, over a broad zone, show overfolding in the opposite direction. The in-dipping axial planes on the southern side of the Alps are more steeply inclined and indicate a much feebler outward movement than on the northern side. But, though very unsymmetrical, the mountain building has been two-sided.

Pyrenees.—Like the Alps, the Pyrenees are an unsymmetrical chain most strongly overfolded toward the north. On the French side conspicuous *nappes de charriage*, with structures dipping southward beneath the range, portray this overfolding toward the north. While these structures attract the most attention and appear dominant, they are only one side of the picture. On the Spanish side the prevalent structures are inclined folds and faults dipping north, indicative of overfolding toward the south on the southern flanks of the chain.[1] The mountain-making mass under compression has flared outward above on both sides, in accordance with the wedge principle. According to Roussel, not only is this true of the main east and west chain, but the old Paleozoic transverse foldings also show a mild turning away from their central axis toward the less disturbed areas.[2] Assuming a wedge, the overflolding is outward toward the margins.

Caucasus.—Similarly the Caucasus display fan structure, so that the crystalline schists on the two flanks dip beneath the gneiss of the central axis and overlie in turn the reversed Paleozoics.[3] Like the Alps and Pyrenees, the chain is unsymmetrical, the overturning toward the north on the northern side being more pronounced than the southward overturning on the southern slopes.

[1] Robert Douvillé, "La peninsule iberique," *Handbuch der Regionalen Geol.*, Vol. III, Pt. III (1911), pp. 143–46; Maurice Lugeon and Nicolas Oulianoff, "Sur la géologie des environs de Camarasa (Catalogne)," *Compt. rend.*, Vol. CLXXIX (1924), pp. 863–66.

[2] Joseph Roussel, *Étude stratigraphique des Pyrénées*, Paris, 1893, pp. 66–70.

[3] Felix Oswald, "A Geologic Map of the Caucasus," Review in *Geol. Mag.* (1914), pp. 371–72; Hagop A. Kharajian, *Regional Geology and Mining of Armenia* (1915), p. 54.

North American Cordilleran Jurasside wedge.—The general easterly-dipping structures of the pre-Cretaceous rocks along the British Columbia coast continue southward in the Cascades, the Klamath Mountains, and the Sierra Nevada.[1] In addition to the inclined folds, Diller has recognized thrust faults which dip eastward, not only near the western border of the Klamath Mountains,[2] but also near the middle of the range.[3] In the Sierra Nevada, the axial planes of the folds in the strongly deformed pre-Cretaceous rocks dip overwhelmingly toward the east.

The Pacific side of the Jurasside orogenic tract is thus readily apparent. But where is the landward side? It must lie to the west of the Rocky Mountains for they were formed later. Along the 49th parallel Daly has recognized strong Jurasside mountain building, accompanied by the characteristic batholithic intrusions, as far east as the Rossland Mountains,[4] which lie just west of the Selkirk system. Farther northwest, near the headwaters of the North Thompson River, the magnificent Cariboo Range is composed of a great mass of mica schists with subordinate gneisses.[5] Viewed from its highest summits, the whole range in latitude 53° N. is seen to have a regional structural dip to the southwest. In detail recumbent folds are abundant, and doubtless there is repetition of the schists by thrust faulting. This region is thought to have participated in the Jurasside orogeny.

But the continuation of the eastern margin of the Jurasside orogeny southward across the United States must at present be left an open question owing to lack of information from the areas where it is to be sought. In southern Nevada, however, Longwell has described strong overthrusting toward the northeast. Thrust faults on a large scale, associated with overturned folds, are found in both the Muddy Mountains and the Spring Mountain Range. These were described as belonging to the post-Jurassic revolution and related to

[1] Many folios, *U.S. Geol. Surv.*

[2] J. S. Diller, *Bull. 546, U.S. Geol. Surv.* (1914), Fig. 4, p. 18.

[3] J. S. Diller, personal communication.

[4] R. A. Daly, "Geology of the North American Cordillera at the Forty-ninth Parallel," *Mem. 38, Geol. Surv. Canada* (1912), Pt. I, pp. 370–76.

[5] R. T. Chamberlin, *Alpine Jour.*, London, Vol. XXXVII (1925), pp. 85–86.

the folding in the Sierra Nevada and Humboldt ranges.[1] If so, this overthrusting toward the east would represent the east flank of the Sierra wedge block, but it is now recognized that this particular disturbance in southern Nevada may have occurred as late as the Tertiary.[2] The details of position and the exact nature of the eastern limb of this apparent wedge must for the present be left largely undetermined.

Kober has grouped together the whole western Cordillera of North America in one orogeny. This makes use of the fact that the Rocky Mountains on their eastern flanks are overthrust toward the east, while along the Pacific Coast the overturning is in the opposite direction. But it does not take account of the fact that the Rockies on one side, and the Sierra Nevada and Pacific Coast ranges on the other side, were formed at totally different times, and are largely independent orogenies. The great period of mountain building in the Sierra Nevada, Klamath Mountains, Cascades, and British Columbia Coast ranges was the close of the Jurassic. The region of the Rockies was then quiet, characterized by sedimentation. The growth of the Rockies occurred at the close of the Cretaceous and during the Eocene. Deformation in the Pacific ranges was then less important. The relationship which Kober has noted is rather a matter of situation and relative position in the North American orogenic frame, than the correlated parts of a single orogenic unit. But it does show that position on one, or the other, of the flanks of a broad general diastrophic tract, like the North American Cordillera, is a matter of much importance in determining the direction of overturning of the structures which develop there. They tend to overturn away from the middle of the diastrophic tract.

NATURE OF WEDGE BORDERS

Any consideration of the nature of the wedge borders brings up several alternative methods of accommodation between the deformed wedge and the less deformed regions adjoining. If the wedge block be marked off by definite shear planes, or zones of active shear,

[1] Chester R. Longwell, "The Muddy Mountain Overthrust in Southeastern Nevada," *Jour. Geol.*, Vol. XXX (1922), pp. 63–72.

[2] Chester R. Longwell, "Thrust Faults and Flaws in Southern Nevada," *Bull. Geol. Soc., Amer.*, Vol. XXXV (1924), pp. 64–65.

the relations would be as shown in Figure 8. Overthrusting in opposite directions occurs on the wedge borders, and the whole block is wedged upward by movement along the bordering shear planes, in addition to the crumpling which takes place within the block. The wedge may be either symmetrical or asymmetrical.

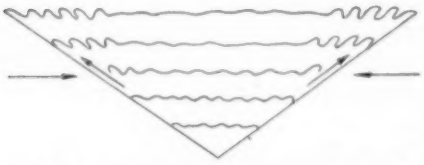

FIG. 8.—Diagrammatical representation of a wedge bordered by shear planes which sharply delimit the block. These are planes of accommodation between the deformed wedge and the less deformed regions adjoining.

On the other hand, the cross-section of a folded belt may very naturally be wedge-shaped without the development of any distinct bordering shear planes, as shown in Figure 9. In this case the folding gradually dies out downward, and all adjustments are gradual transitions. With most ready yielding near the surface, it is natural for the folded zone to become narrower below, owing to the increasing difficulty of deformation with increasing depth. This produces the wedge shape.

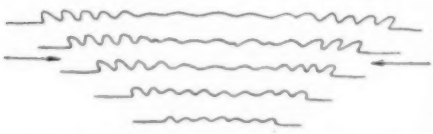

FIG. 9.—Wedge shape by folding alone. As the folding dies out downward the folded zone becomes narrower. The folds are diagrammatic, making the case simpler than it would probably be in nature.

FIG. 10.—A wedge bordered sharply on one side by shear planes while the folding dies out more gradually on the other side.

FIG. 10*a*.—Similar to Figure 10, but with more pronounced shearing. This grades into a one-sided orogeny.

Figure 10 illustrates a combination of the two previous cases. The wedge is here bordered on one side by shear planes, while on the other side the folding dies out gradually. Such

a deformed mass would normally be unsymmetrical, the greatest overturning being on the side of the shear planes. Figure 10*a* presents a one-sided orogeny in which extensive horizontal shearing finally rises to the surface.

In Figure 11 the wedge is bordered by shearing planes above but, with increasing depth, shearing gives way to more gradual accommodation between the deformed block and the less deformed region outside. This may be by the development of cleavage or schistosity, or by more pervasive rock flow without the development of these structures. In this wedge both margins are overfolded and overthrust in opposite directions, as are so many cases in Nature. The wedge may or may not be symmetrical, depending upon local conditions.

FIG. 11.—A wedge bordered by shearing planes above, but with more gradual accommodation below. In the actual case (as also in Fig. 8) there would normally be several thrust faults on each wing in addition to the border faults. These are left out for the sake of simplicity. This is probably the most typical of all the wedges.

SUCCESSIVE CHANGES IN A WEDGE BORDER

As the compression of a mountain wedge progresses and the folding within it sharpens, its width correspondingly decreases. With much crustal shortening, the shape of the wedge should change notably, if there were no additional developments. The bordering shear zones should become steeper and steeper as the two margins are driven closer and closer together. But a study of the development of the boundary faults on the overthrust margins of various mountain systems brings out the fact that the boundary faults of the early stages in the growth of a mountain system are not the boundary faults of the later stages. Detailed work on the Scottish Highlands[1] and the Rocky Mountain front in Montana and Alberta,[2] to cite only two very beautiful expressions of the principle, indicate

[1] J. Horne, "The Geological Structure of the Northwest Highlands of Scotland," *Mem. Geol. Survey of Great Britain* (1907), pp. 471–76.

[2] J. D. MacKenzie, "The Historical and Structural Geology of the Southernmost Rocky Mountains of Canada," *Trans. Roy. Soc. Can.*, Vol. XVI (1922), pp. 122–30.

conclusively that after considerable overthrusting has occurred along a given major thrust plane, minor folding and a series of minor slice faults develop in the previously undeformed region just outside the belt of growing mountains. With further compression, a new major overthrust fault breaks through the foothill zone of minor folds and slice faults, and comes to the surface out on the plains beyond. This becomes the new boundary fault of the mountain wedge-block, and the mountainous area and the wedge are widened by this addition (see Fig. 4).

In strongly deformed ranges, the process has been repeated several times. In the Scottish Highlands the succession of the Moine, Ben More, Glencoul, and other major thrusts, each one below and extending farther out than its predecessor, pictures clearly the successive forward marches of the imbricate structure into the less deformed belts flanking the growing mountains. With each episode of spreading of the imbricate structure, and the breaking out and overriding of the whole mass of slices along a new "sole" or major thrust plane, the compressed wedge has been widened to offset in part the crustal shortening within it. Thus the general wedge shape is maintained by successive additions to the orogenic tract, and the wedge becomes increasingly complex in structure.

II. PLATEAU UPLIFTS

Some folded mountain systems, particularly of the thick-shell type, grade directly into plateau uplifts,[1] while certain other mountain systems, even of the more intensely deformed sort, manifest sufficiently close relationship to adjoining plateaus to suggest genetic relationships. They suggest the extension of wedge dynamics to the uplift of plateaus as a working principle, though it is to be recognized that plateaus are the least sharply defined of the larger diastrophic units of the globe and are not all formed in the same way.

At the present time the two greatest plateaus of the globe are Tibet and the Bolivian Plateau. Both are high tablelands inclosed by lofty mountain ranges, Tibet being the elevated region between the Himalayas and the Kuen Lun group of ranges, while the Bolivian

[1] "The Building of the Colorado Rockies," *Jour. Geol.*, Vol. XXVII (1919), p. 249.

Altaplanicie lies between the chains of Eastern and Western Andes. In somewhat similar fashion, the Mexican Plateau lies between the Sierra Madre Oriental and the Sierra Madre Occidental.

In each case the folding of these plateau tracts has been along axes having trends similar to the trends of the folding in the mountains adjoining. But in each case it is to be noted that the strata of the plateau areas have undergone much less folding and thrust faulting than the strata of the bordering mountain belts. The amount of crustal shortening in later geologic times in these plateaus has not been great, while the amount of uplifting has been impressive. Gentle folding with strong uplift indicates deformation of the thick-shell type. Crustal deformation has apparently reached to greater depths under the plateaus than beneath the more strongly compressed mountain ranges adjoining. This suggests that possibly the mountain chains bordering the plateau constitute the strongly deformed shallow marginal portions of a large wedge, while the plateau comprises the much less deformed deep middle portion of the wedge-block. Kober's grouping of Randketten and Zwischengebirge is another way of expressing this relationship. On the other hand the two marginal mountain belts may constitute two independent mountain wedges and the plateau a separate unit between.

After cessation of the more intense phase of deformation, a milder recurrence of the stress conditions distributed through a shell deeper than before, may further elevate both the plateau and the previously folded mountain chains with but little additional warping. Or, much later, following extensive denudation and the removal of much material, the whole previously deformed region may be re-elevated with little internal deformation, converting the former mountain tract into a plateau.

On the other hand, areas of flat-lying sedimentary formations may be raised to plateaus without notable internal distortion, either wedged up by faulting on the margins, or in other cases probably entirely independently of any wedge action. In Figure 12 a wedge-like portion of a continental block has been uplifted with but little internal deformation. Uplift has been greatest near the continental margin, diminishing slowly toward the middle of the continent. Southern Brazil and Uruguay exhibit such a relationship; the Serra

do Mar is a plateau-scarp rising precipitously from the Atlantic; long cuesta slopes descend gradually westward to the Rio Uruguay and the Rio Paraná. Similarly in southeastern Australia the rise to the

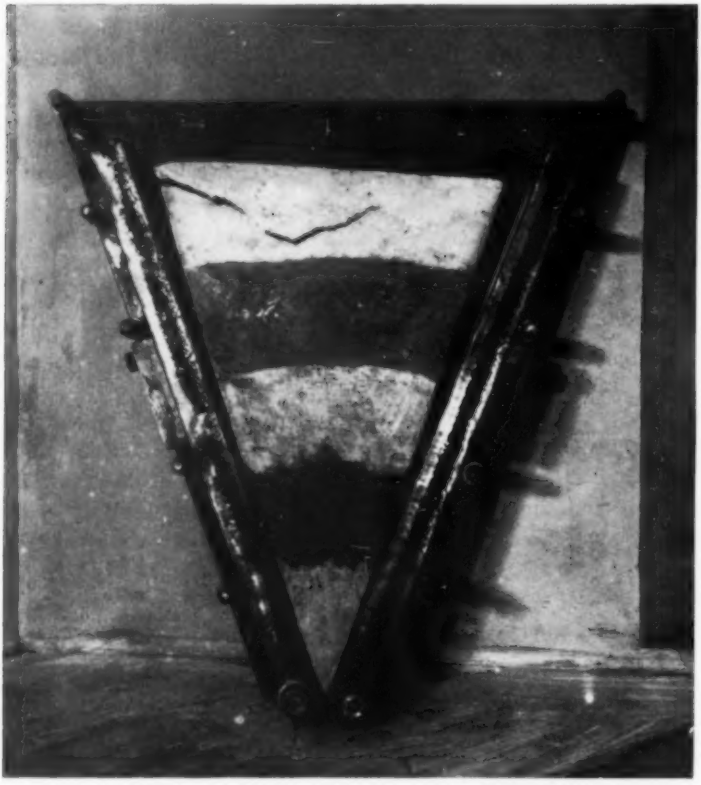

FIG. 12.—Wedging up one side of a continent to form a plateau

Blue Mountain plateau is a steep upturn not far back from the coast; the big rivers flow westward down long slopes away from the ocean. The Abyssinian Plateau apparently is somewhat analogous. While other alternative explanations than the wedge theory may fit these cases, they are quite in accord with its principles.

If a broad plateau, like Tibet, comprises a wedge block, such as postulated for narrower mountain chains, its sides projected downward would meet at a depth of 300 miles beneath the surface. If only moderate folding extended to a depth of 300 miles in the middle of the plateau, the upward bulge resulting from the shortening would be greater than the observed uplift of Tibet. The advocates of the doctrine of isostasy will doubtless at once contend that the shortage of bulge in the middle of the plateau is readily explained by the postulates of the isostatic theory. Independently of this, it would point definitely to the conclusion that folding of the sort seen at the surface did not extend so deep, and that the type of adjustment at these depths was of a different nature. What manner of adjustment might be expected to prevail at these depths? The theory, followed in this paper, that the operating cause of the diastrophism is the reorganization of material in favor of greater density in the interior of the earth, affords a ready answer. Folding, faulting, and the general wedge principle are the workings of the less active surficial portion of the globe; beneath this, in the more active zone, shrinkage by general condensation is the *modus operandum*. No bulge results from this. Thus in the middle deeper portion of a large block, like Tibet, general reorganization of material would gradually replace the wedge operations which govern the behavior of the upper portion of the block. While alternative hypotheses may be tenable, the very lack of adequate bulge in the middle portion of a broad plateau, or continent, which was originally regarded as an objection to the wedge theory, really points toward the basal postulates of the wedge theory. Observational evidence from this new angle, therefore, helps support the theory. Let us turn next to continental masses.

III. THE WEDGING UP OF CONTINENTS

As brought out in earlier papers, the principle of the wedge-shaped deformed block seemed to find useful application in the behavior of continental masses.[1] The geologic record emphasizes the fact that, from time to time in the long history of the globe, the continental areas have been uplifted and the shallow seas, which had

[1] Rollin T. Chamberlin, "The Intimations of Shell Deformation," *Jour. Geol.*, Vol. XXIX (1921), pp. 416–25; "The Significance of the Framework of the Continents," *ibid.*, Vol. XXXII (1924), pp. 545–74.

spread partially over them during periods of relative quiescence, have been forced to withdraw. The general nature and mechanism of the uplifting movements which restore continents from time to time have been discussed at considerable length.[1] The continental and sub-oceanic masses have been treated as segments, the sub-oceanic masses being the master segments of greater size and heavier rocks, and the continental segments being smaller and of lighter rocks.[2] The principal source of the deforming forces has been taken to be rearrangement of material in the interior of the globe in favor of greater compactness and higher density. This should cause general shrinkage with resulting circumferential compressive stresses throughout the entire peripheral portion of the spheroid, though not reaching the same intensity everywhere. Strong tangential thrusting stresses would develop beneath both the continental and oceanic areas.

In the shrinking process all segments would sink, but the heavier and more massive oceanic segments would take the lead and settle most in consequence of their greater density and because of the upsetting of isostatic equilibrium by denudation of the land areas and aggradation of the sea bottom. As the master segments take precedence in sinking into a smaller circumference, they squeeze the smaller, lighter continental segments between them, so that the latter not only sink less, but, relative to the oceanic segments, are wedged upward. The general relations have been expressed diagrammatically in Figure 13. This is the wedge theory applied to continents.

SEGMENTAL CONTACT ZONES

If the continental wedge be outlined by actual fracturing, the borders, as shown by the familiar crushing-strength tests upon building stones, as well as an analysis of the stress-strain relations, should be fault planes dipping beneath the continent at angles in the general vicinity of 45°, though in most cases somewhat less.[3] Consider-

[1] *Jour. Geol.*, Vol. XXXII (1924), pp. 548–53.

[2] T. C. Chamberlin and R. D. Salisbury, *Geology*, Vol. I (1904), pp. 520–24; T. C. Chamberlin, *The Origin of the Earth* (1916), chap. viii, pp. 159–225.

[3] This is on the assumption that the strain was non-rotational; if rotational, the angles of the border planes will be lowered from 45° in proportion to the extent of the rotational element.

ing the magnitude of the masses involved, a broad zone of slippage planes should be expected to replace the single fracture plane of small-scale building-stone tests. Actual fracture and thrust faulting should occur most prevalently near the surface. With increasing depth below the surface actual faulting should diminish, though distributive shear should presumably descend much deeper. At greater depths, movement by molecular rearrangement and recrystallization presumably should take precedence. This might be manifested by folding, the development of cleavage, or perhaps only by rock flow.

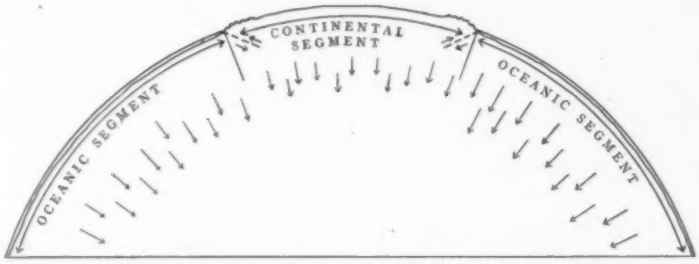

FIG. 13.—Diagram to illustrate deformation of a continent. Arrows pointing toward center of earth indicate general condensation. Long arrows represent tangential compressive stress. All segments sink, but the continent is wedged up somewhat relative to the oceanic segments. Dash lines represent shear under the continents. Vertical lines represent relations of segments before diastrophism. (From *Jour. of Geol.*, Vol. XXXII [1924], p. 552.)

Under such conditions adjustment between segments should be more distributive and the deformed continental block would probably not be sharply bordered. Broad zones of accommodation characterized by shearing above, and more pervasive adjustment below, should separate the continental and oceanic segments.

In Figure 13 the functioning shear planes of the continental wedge are inclined at 45°. If projected downward at this angle until they meet, the outlined continental wedge will be found to be comparatively shallow and to comprise but a small portion of the globe. If, instead, the bounding planes are drawn so as to cut each radius of the sphere at 45°, the continental wedges reach somewhat greater depths.[1] Under the hypothesis of earth accretion, the differentiation

[1] *Jour. Geol.*, Vol. XXIX (1921), Fig. 1, p. 420.

into continental protuberances and ocean basins took place early in the growth of the globe. The consequent differentiation of materials by gradational and other processes, and the contact zones between segments in the growing stages of the earth, should have affected horizons now lying at great depths. Such vestiges of the growing stages should not be lost sight of in the problems of megadiastrophism. Inheritances from these early stages may still play some part. But, as suggested under the topic of deep plateau wedges, the lack of strong uplift in the middle portions of continents points toward the conclusion that the adjustments in the deeper reaches are accomplished in a manner different from those nearer the surface. Because of this and because of the complexity of the case, the boundary planes of the continental wedge in Figure 13 were not projected far below the surface.

ANALYSIS OF THE CONTINENTS

MARGINAL FRAMEWORK

As brought out in "The Framework of the Continents," the typical continent is characterized by a bordering frame of folded and faulted structures while, in general, the great interior of the continent preserves Paleozoic and later sedimentary beds, flat-lying over vast areas.[1] The margins of the continents at the present time are either composed of old rocks, for the most part strongly folded and metamorphosed, or else a coastal-plain series of undeformed Mesozoic and Cenozoic sediments which are younger than the last strong deformation of the region. These young, undeformed sedimentaries simply indicate a quiescent condition of such portions of the continental border in later times; the deformation of that particular border has occurred earlier. Undeformed rocks as old as the Paleozoic, which are so characteristic of the interior of the continents, are conspicuously absent from the borders. This is striking and significant. The structural trends are prevailingly parallel to, or concentric with, the general outlines of the continents. Intrusions of igneous rock are particularly abundant in these deformed border tracts; injected into folded structures, their long dimensions are

[1] *Jour. Geol.*, Vol. XXXII (1924), pp. 553–61.

naturally also parallel to both the mountain folds and the edge of the continental platform. This relationship of structure, metamorphism, and igneous activity to the outlines of the continental segments cannot be fortuitous; it must be a relation of prime importance.

The possible working hypothesis that the belts of mountain folding are the primary features and that the outlines of the continents—the earth's major relief features—are secondary derivatives, does not appear a fruitful line to follow. It might perhaps be urged that folding and faulting first established the structural trends and general grain of a region, whereupon down-faulting of large areas along fracture lines parallel to the structural trend, developed ocean basins whose shores would be parallel to the grain of the adjoining land area. But the answer to this is that shallow-water marine sediments and widespread terrestrial deposits indicate that the present continents were continental areas from very early geologic times, and that generation after generation of mountains were folded on their borders all through geologic time. With the necessity of deep basins to hold the waters of the globe, such basins must, for want of any other possible locations, have been essentially where they are today. The continents being very ancient, as shown by their sedimentary rocks and the antiquity of their present border belts, the foundering of coasts parallel to the structural trends of the continental borderlands in later geologic time can only be a subordinate matter whose chief importance would be in modifying local details. Similarly, differential erosion of the linear belts of unequally resistant rock may tend to keep the coasts parallel to the structural trend, but this is hardly the origin of the main outlines.

The general morphology of the typical continent, as outlined above, finds a ready explanation in the wedge theory and the general philosophy of which it is a part. In the wedging up of a continent the greatest intensity of thrusting will be located on the borders of the compressed wedge-like block. Compressive stresses will affect the entire block, but their intensity will be less away from the borders. Yielding by folding and faulting will therefore be strongly predisposed to occur along the borders of the squeezed block, and in general parallel to the borders, while the interior of the wedge block

will be much less affected as a general rule. Certain other factors which contribute to this general result have been discussed in some detail elsewhere.[1]

The result of the up-wedging is the restoration of the continent, the withdrawal of the epicontinental seas, and the strengthening of the relief of the continental platform by the up-bowing of the peripheral mountain framework. In each episode of diastrophism, however, the different oceanic segments do not play an equally active part. In later geologic times, the Pacific segment has been more active than the Atlantic. Furthermore, yielding is commonly not the same on opposite shores of the same ocean. The Caledonian deformation at the close of the Silurian affected western Europe vastly more than eastern North America, while the Acadian deformation in Upper Devonian times was more severe on the North American shores than on the European side. The marginal framework of the continents thus develops piecemeal in successive deformative episodes.

That the piecemeal development of the frame, however, often takes place in large sections is illustrated by the American Cordillera in which essentially simultaneous folding occurred throughout the Pacific sides of both the North and South American continents, amounting to approximately one-third of the entire circumference of the globe.[2] The development of this vast chain, all along the eastern border of the Pacific, indicates that a large part of the globe was involved in the growth of the deformative forces. The units were large, and very different from the tiny units dealt with by the ordinary form of the isostatic theory, and some other theories which emphasize an extremely local type of adjustive movement.

PHASES ASSUMED BY IN-DIPPING STRUCTURES ON CONTINENTAL BORDERS

In the belts of strongly deformed rocks which characterize so generally the coastal regions, the strike of the bedding, the schistosity, and the lines of folding and faulting are prevailingly parallel to the coasts. Near the coasts these structures dip prevailingly in under

[1] *Jour. Geol.*, Vol. XXXII (1924), pp. 551–53, 566–72.

[2] In addition there is related folding in Antarctica and northeastern Asia.

the continents. This in-dipping schistosity is particularly conspicuous all along the American Pacific Coast from Alaska to Puget Sound, and at other points farther south. The corresponding relations on the Atlantic Coast of the United States have been expressed by Woodworth as follows:

> The westwardly dipping schists along the Atlantic coast from southern Maine southward across Maryland and North Carolina, as seen beneath the cover of Mesozoic and Cenozoic strata along the inner edge of the coastal plain, support the view of a widespread overturning of structure toward the southeast, or, in other words, toward the present Atlantic basin.[1]

The in-dipping structures are characteristic.

The direction of inclination of the structure planes near the margins of the continents is apparently a function of their location. Several alternative hypotheses are possible.

I. Continental creep, in which the continental masses spread outward glacier-like. The shear planes in a glacier tongue strike closely parallel to the curving margin of the glacier, and dip regularly inward toward the middle of the ice tongue. Unless the ice tongue encounters strong resistance on the sides, from rock walls or another ice tongue, there is no deviation from this general rule. To some extent the continents probably spread outward in similar fashion. But in the case of continents such movement would seem to be of a very mild sort. It is too much to expect it to cause strong folding and metamorphism of the rocks sufficiently intense to develop cleavage.

II. The in-dipping schistosity may be the surface expression of the broad shear zone between the continental and oceanic segments.

III. The in-dipping schistosity may represent the seaward side of a coastal mountain wedge-block.

These last two possibilities are closely related and an actual blending of the two is to be expected near the outermost border of the continents. Since these belts of inwardly-dipping schistosity have considerable width, and are characterized by strongly developed folding, it is probable that they represent chiefly border mountain-wedge structures. The zones of accommodation between segments are probably largely submerged beneath the shelf seas.

[1] J. B. Woodworth, "Cross-Section of the Appalachians in Southern New England," *Bull. Geol. Soc. Amer.*, Vol. XXXIV (1923), p. 258.

MOUNTAIN WEDGES COMMONLY NOT SYMMETRICAL

Most mountain wedges are found to be unsymmetrical. Mountain ranges in many parts of the globe afford excellent illustrations of border faults on their inland margins. Such are the great overthrusts bordering the east side of the Rockies and eastern Andes in many places; on the west side of the Appalachians, the north side of the Alps, and other less striking cases. On the other hand, border faults on the seaward side of mountain ranges have been far less commonly found. These on the southern flank of the Himalayas are perhaps the most conspicuous case.

In the western Appalachians, where border faults characterize the inland flanks of the folded belt in many places from Quebec to Alabama, the involved strata are as a rule but little metamorphosed, except in the north where the case has been complicated by earlier periods of folding. On the other hand, the whole seaward half of this wedge block is composed of strongly metamorphosed rocks whose structures are unraveled with difficulty. Border faults on that side would be difficult to recognize and may be beneath the ocean. The difference in degree of metamorphism in the two halves is striking and significant.

In western North America the great overthrusts of Montana and Alberta are the boundary faults on the inland side of the Rockies. They bring gently folded Proterozoic quartzite and slightly metamorphosed Paleozoic strata out over soft Cretaceous formations. On the west side of the local Rocky Mountain wedge some in-dipping faults have developed, but they are of lesser magnitude than the overthrusts on the edge of the Great Plains.

In the Klamath-Sierra Nevada wedge the well-developed thrust fault on the seaward side of the Klamath Range, which Diller has mapped, hardly attains the magnitude of a great overthrust, and judging from the overthrusts in southern Nevada, further field work is likely to reveal more pronounced thrusts on the inland side than on the seaward margin. At the same time metamorphism has been more pronounced on the seaward side. In the coastal region of British Columbia and southeastern Alaska, noted for schistose rocks dipping landward, border faults have not been recognized, though such may exist beneath the ocean, as stated for the eastern Appalachians.

This coastal strip also has been a region of much igneous and metamorphic activity.

The Andes exhibit similar relationships. On the inland margin of the Eastern Andes in Bolivia, Rocky Mountain structure and relationships are repeated. The overturning is away from the ocean, the thrust faults dip westward beneath the folded range, and the strata have suffered little metamorphism.[1] On the seaward side of the Western Andes distinct border faults are not apparent, while igneous activity and metamorphism complicate the problem.

In general, the deformed strata on the seaward side of a marginal wedge are commonly metamorphic; on the landward side they are rarely so. Border thrust faults are well shown on the landward side; on the seaward side they are far less commonly seen. These notable differences emphasize the generalization already made, that the margin of the continental block plays a very important part in mountain building. In the compression of a continental wedge, it is natural, for reasons discussed elsewhere, that the margins should be most strongly deformed and most frequently deformed. Folded belts in the form of mountain wedges are disposed to develop near the borders of the continental wedge rather than in the interior. For the same reasons the folded mountain belt is more strongly deformed and more metamorphosed on the side of the continental border than on the inland side. In addition, if the folded belt is very broad, and the result of several episodes of deformation, the more active oceanward side may have participated in more orogenic episodes than the landward side. Some of the differences in metamorphism may be accounted for in this way.

In the development of asymmetry in a mountain system near the border of a continent the distribution of forces and resistances will govern. While much of the compressive stress which deforms a continent is developed within the continental segment itself, still the overbalancing force comes commonly from the adjacent oceanic segment. Thrusting is therefore more active from this side than from the other, though coming from both sides. As overthrusting is mechanically easier than underthrusting, there is a theoretical

[1] Kirtley F. Mather, "Front Ranges of the Andes between Santa Cruz, Bolivia, and Embarcacion, Argentina," *Bull. Geol. Soc. Amer.*, Vol. XXXIII (1922), pp. 703–64.

reason to expect greater overturning and greater thrust faults on the inland margin of a wedge mass, where the relationship is that of overthrusting by the greater force, rather than on the seaward side where the relation is that of overthrusting by the lesser force, or underthrusting by the greater force. Likewise the belt of folds overturned away from the ocean theoretically should be wider and more conspicuous than the belt of folds overturned toward the ocean.

In respect to attitude, there is a further consideration of very wide application. The continental border stands high above the oceanic floor and there is a strong upturn from the latter to the former. Thus the thrusting from the ocean floor is applied at a lower level than the corresponding thrust from the continent, and this gives a tendency toward underthrusting from the ocean, coupled with an overturning of the surface structures toward the ocean. Any outward creep of the surficial portion of the continent accentuates this tendency.

From these considerations it appears that position relative to the continent is a factor of much importance in determining the overturning. Thus it is that mountain systems far apart, like the Rockies and the Pacific Coast ranges, which Kober has grouped together, may owe their oppositely inclined structures primarily to their relative positions in the continent, and may be largely independent of one another. They need not be correlatives in other sense than that, and may be developed at entirely different times. The Himalayas and the Kuen Lun-Nan Shan group may be only correlatives in a similar sense, but this is not affirmed for lack of knowledge, and their development may have gone hand in hand with the wedging up of the great Tibetan mass. Such an extensive upthrusting as the Tibetan Plateau would develop a tendency to move outward above. Overthrusting from the middle of the squeezed mass outwardly on each flank would give relatively easy horizontal relief after the upward yielding had attained certain proportions. Perhaps the mountains represent only the shallow marginal manifestations of a wedge which loses its distinctiveness with increasing depth beneath the mass of Tibet. Further knowledge of time relations and other relationships of these very important ranges is greatly needed.

Kober's general view of the origin of folded mountain ranges is that they represent geosynclinal tracts of sedimentation squeezed between two adjoining tracts of more resistant rocks called forelands. The weaker sedimentaries tend to be thrust up onto the two adjacent forelands. Stille and others in central Europe are now urging essentially the same idea.[1] In this view the forelands play a rôle of major importance, scarcely subordinate to that of the geosyncline. This view, therefore, is less a matter of general wedge mechanics than it is of the simple squeezing of a weak tract between two sharply defined stronger tracts.

As brought out in a previous paper, a typical geosynclinal tract should be flanked on one side by a corresponding geanticlinal strip to furnish the great mass of sediment required to fill the geosyncline. The two are correlatives and together form a geanticlinal-geosynclinal couple.[2] While the geanticline is on one side, there is nothing on the opposite side of the geosyncline, in such typical cases as the west margin of the Appalachians and the east margin of the Rocky Mountains, that can properly be called a foreland in any genetic sense. In these cases the sediments attained their greatest thickness in the portion of the geosyncline adjacent to the geanticline and merely thinned away very gradually for long distances into the interior of the continent, where they seem to have taken no part in the orogenic movements. The very sharply defined inland margins of both the Appalachians and Rockies are not matched by any similar sharp feature in the sediments. Something deeper and more effective seems to be implied.

In the judgment of the writer, the unusually regular, orderly-built American continents offer a better field for the unraveling of the major problems of geology, than the much more irregular, tortuous, and complex formations of Europe. Kober's belief that the typical orogeny is a folded belt between definite forelands seems to the writer to have been based on a model that is not quite typical, and certainly not sharply in accord with the more regular American mountain systems.

[1] Hans Stille, *Grundfragen der Vergleichenden Tektonik* (1924), pp. 259–80.

[2] *Jour. Geol.*, Vol. XXXII (1924), pp. 568–69.

THE PART PLAYED BY MAGMAS

Magmas are the variables in the problem. Nevertheless it is a matter of world-wide observation that bulky intrusions, particularly of light, acidic magmas, have commonly entered the middle portions of strongly folded mountain systems. These plutonic invasions have been too general and persistent to be fortuitous. They must be related to the folding.

It is apparent that in the upfolding of a great mountain range there are situations beneath strongly arched portions of the folded belt where, in consequence of the arching, a partial relief of pressure occurs. In laboratory experiments, sizeable cavities often develop beneath anticlines; in nature, cavities would oftener be potential than actual, since under the prevailing pressures material forced in from the sides and from below would usually prevent their development. Partial relief of pressure, however, would be likely to develop at some points and would favor liquefaction, if the material were already sufficiently near the melting point. Liquefaction may also be assigned to the reorganizing processes to which the orogeny itself is referred since these processes are held to have been such as to change a part of the pre-existing substances into minerals of greater insolubility under the existing conditions, and a part into a state of mutual solution. The liquid factor in the reorganized mass would facilitate whatever movement was in progress, and would itself be specially susceptible of being forced to the surface.

Such magmas, or any other magmas already existent or concurrently produced, whatever their origin, would be subject to the squeezing effect of mountain deformation. Differential pressure, compressive in some places and relaxitive in others, would facilitate the movement of magmas, for liquids are able to insinuate themselves into any path that offers the least resistance. The most available paths of escape from pressure would be upward, for the most part. Diminution of pressure beneath the most strongly uparched portions of the range would help concentrate magmas in those portions. This would favor the formation of batholiths which are conspicuous as igneous cores in many strongly folded mountain systems.

This leads to the question: How important an agent can magmas become in promoting further deformation? Being under hydrostatic pressure, and possessing greater mobility than the associated solid rock, squeezing forces in operation at considerable depths where all forces are great, may, through transmission by connected magmas, more readily become effective in places nearer the surface where the resistance is much less. This natural hydrostatic press may have considerable possibilities.

On the other hand, there is danger of attributing too much deformation to the thrusts of invading magmas. Keith has presented arguments in support of the hypothesis that the folding of the Appalachian Mountains was the work of forces arising directly from batholithic intrusions in the heart of the range.[1] He has pointed out that the greatest salients and the most pronounced overthrust faults are situated opposite the greatest batholiths. A relationship is evident, but we may perhaps still wonder which is cause and which is effect.

Keith says: "If, therefore, the Appalachians as a whole were shortened 40 per cent in a northwest-southeast direction, it is evident that the present visible width of 270 miles, plus 30 miles for probable extension under the Cretaceous, represents an original width of 500 miles and that the amount of shortening of the crust is thus 200 miles."[2] The picture here is that of horizontal compression of the folded belt by forces acting from without. Five hundred miles of surface formations have been squeezed into 300 miles of present width. But the batholiths are in the heart of the folded belt. Any thrusting from these intrusions should be directed from the middle of the folded belt outward.[3] If the folding were solely caused by these batholithic intrusions in the middle portion of the range, the position of points outside the deformed zone would remain essentially unchanged, and there would be no general crustal shortening.

On the batholithic intrusion hypothesis, regarded as the essential cause of Appalachian folding, the greatest width of surface

[1] Arthur Keith, "Outlines of Appalachian Structure," *Bull. Geol. Soc. Amer.*, Vol. XXXIV (1923), pp. 309–80.

[2] *Op. cit.*, p. 335.

[3] Except locally between separate batholiths.

formations in the folded belt at any time is the width of the folded belt itself. For the complete Appalachian tract, 300 miles across, there would be 300 miles of sedimentary strata as originally laid down. No strata deposited outside the present 300-mile belt need be considered, for, on this hypothesis, there is no force to crowd surface rocks into this belt from the outside. The forces operate in the opposite direction.

If the 300 miles of flat-lying sedimentary beds be folded, while the width of the belt remains constant at 300 miles, the folding and horizontal shortening of the surface strata within the belt must obviously be compensated by the introduction of subsurface material. The igneous intrusions would probably be required to supply most of this. Using Keith's estimate that the folding indicates a shortening of at least 40 per cent, the 300 miles of geosynclinal sediments would have been compressed into 180 miles of folded belt, while the remaining 120 miles would need to be occupied by subsurface material, largely igneous. No mere intrusion of batholiths under cover rocks meets the case. If the shortening of 40 per cent, now observed in the eroded stumps of the range, continued undiminished to the surface of the original folds, a pulling apart and pushing apart of the surface itself to the extent of 120 miles of gap would be required.

But batholiths in general underlie a roof-cover until it is removed later by erosion, the folded sedimentary strata, even in their greatly contorted condition, still being areally extensive enough to surface the deformed belt. One of the outstanding features of the Alps is the important part played by Cenozoic and Mesozoic sediments in the extraordinarily complicated *nappes de recouvrement*. Paleozoic rocks become more abundant in the region of the roots, but igneous rocks, contemporaneous with the folding, are of comparatively limited extent. The marvelous contortions are of strata which were near the surface at the time of deformation. Instead of great gaps to be filled in by igneous rocks as the batholithic hypothesis would require, these geologically young strata are doubled back on themselves repeatedly in overthrusts and recumbent folds, so that if drawn out flat again they would cover vastly more area than that

of the folded Alpine tract. They must have been jammed into their present crowded space by compressive forces from without.

On the other hand, while some large batholithic masses have reached their present position without exhibiting any notable thrusting effects,[1] in many other cases there is evidence of considerable crumpling of invaded strata near the igneous contact. Some folding and thrust faulting of strata, though not too much, may thus be attributed to igneous intrusions.

Masses of magma deep down may be subject to greater horizontal compression than the strata which are being folded nearer the surface. On the hydrostatic principle, if the fluidity is great, this pressure will be transmitted to portions of the same liquid mass near the surface when, in the later stages of the deformation, large batholiths develop in the strongly folded portions of an orogenic belt. The portion of the magma near the surface may have considerable thrusting power derived from below and still be a liquid. At greater depths the increased strength of the rocks due to cubic compression, on Adam's principle, resists lateral thrusting; nearer the surface where lateral resistance is less, the force transmitted by the liquid magma may, in certain cases, be sufficient to accomplish notable lateral movement of the adjoining rocks.

One of the notable features of strongly folded mountain systems is the driving outward of rock sheets along low-angle thrust planes on the two margins of the orogenic wedge in the late stages of the deformation. Special stress conditions producing rotational strain are the prime factors in causing these low-angle overthrusts.[2] Increasing the lateral compressive stress above, relative to that below, while the resistances remain unchanged, is one of the ways of developing the appropriate type of rotational strain for the overthrusts. A large batholithic intrusion, by increasing the thrusting near the surface, might help develop the rotational stress which produces great overthrusts. In addition, as the world's batholiths become better

[1] Adolph Knopf, "A Geologic Reconnaissance of the Inyo Range and the Eastern Slope of the Southern Sierra Nevada, California," *U.S. Geol. Surv. Prof. Paper 110* (1918), pp. 12, 62–63.

[2] R. T. Chamberlin and W. Z. Miller, "Low-Angle Faulting," *Jour. Geol.*, Vol. XXVI (1918), pp. 43–44.

known, it is probable that more and more of them will be found to be horizontal pancake-like masses of the general laccolithic shape, which locally, at least, at the time of intrusion, would serve to detach the overlying rocks from those beneath, with a "sap-layer," or layer of easy accommodation, between. This, both by lifting the overlying rock shell, and by reducing friction to horizontal sliding beneath it, could well become a co-operating factor in facilitating the overthrusting of the upper rock shell.

The batholiths characteristically do not appear among the overthrust slices out on the wedge margins, but are confined largely to the middle portions of the deformed mass, some distance behind the overthrusts. If the batholithic intrusions play a part in the overthrusting, the transmission of their thrusts for considerable distances would seem necessary. Further study is needed to determine whether or not this is a serious objection. But Keith's observation that the greatest overthrusts in the Appalachians are opposite the greatest intrusions is very suggestive.[1]

The rôle of intrusions in the wedge theory is thus entirely secondary. They appear in the middle portion of the mountain wedge in a late stage of the deformation. But when appearing in large volume, they may become an important auxiliary agent in helping to drive the overthrust slices outward toward the margins of the wedge-block, and in making more pronounced the outward flaring of the folded structures on the opposite sides of the wedge.

[1] See also Bailey Willis, "Rocky Mountain Structure," *Jour. Geol.*, Vol. XXXIII (1925), p. 275.

EXFOLIATION AS A PHASE OF ROCK WEATHERING

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ABSTRACT

By the scaling off of side corners and edges, blocks of massive rocks commonly develop into bowlders. In geologic literature this type of weathering has been rather generally ascribed to alternating heat and cold. In this paper the various current explanations are considered, and the conclusion is reached that hydration and similar chemical processes, rather than temperature changes, are the principal causes of exfoliation.

It is a matter of common observation that certain massive rocks tend to scale off along joints, especially at corners and edges, and thereby develop bowlder-like forms, which are often inclosed in concentric shells. The process is most common in the granitoid igneous rocks, and it occurs under various conditions.

EXPLANATIONS GIVEN BY AUTHORS

The phenomena have been described by many writers during the last seventy-five years and have been interpreted in various ways. In more than thirty standard reference works and special papers on weathering I find these explanations suggested or declared: (1) tectonic stresses; (2) cooling stresses in solidifying magma; (3) expansion due to relief of vertical compression caused by the lowering of the surface during erosion. (This explanation was offered by G. K. Gilbert, with reference to the dome-shaped joints in the Yosemite granite. The curved shells thus made are very much thicker than in exfoliation due to weathering.) (4) Crystallization of salts in the pores between crystals; (5) chemical alteration of the rock minerals resulting in expansion; (6) temperature changes. These may be either seasonal, daily, or sudden. The great majority of writers refer exfoliation to daily range of temperature in deserts and on mountain tops, where the range is exceptionally large. One author emphasizes the importance of dashes of cold rain during the passage of local showers across highly heated rocks in desert regions. A few writers regard seasonal changes as having important effects.

In the field I have found serious difficulty in reconciling some of these explanations with the observed facts, and this led me to re-examine the general subject. As a result I have concluded that ex-

FIG. 1.—Bowlders developed in massive granite by exfoliation along joints, Alabama Hills, Owens Valley, California. There is reason to believe that these hills were formerly covered with residual soil which has been swept away in consequence of a change to a more arid climate.

foliation is not a simple process, but several processes of diverse origins, and that it has been rather generally misinterpreted, especially by the writers of current textbooks.

THE PROCESS IN GENERAL

In typical exfoliation a series of cracks is formed, roughly concentric to a single joint-block. This indicates that the force producing the cracks has been one of tension, with the effective component operating in a radial direction. Also, it is hardly necessary to say that the force must have been strong enough to disrupt the rock. Such radial forces originate either (*a*) by the internal shrinkage of the block or (*b*) by its external expansion.

I have seen no explanation of stresses due to earth movements or

diastrophism that could produce spheroidal cracks, nor can I imagine a suitable case. Merely curved joints are not true exfoliate structures. For this reason I think it unnecessary to consider here the origin of sheeting structure in granite and of the dome-shaped joints of such regions as the Yosemite Valley in California. Whether these be due to the relief of compression through erosion, as suggested by G. K. Gilbert,[1] or to some other cause, they are structures of a larger order of magnitude than the exfoliation of individual joint-blocks.

Nearly all changes in rocks proceed from the exterior of the block inward. A few, such as heat conduction and radium emanation, may have the reverse trend. However, I know of no probable internal change in a block at or near the surface that would cause shrinkage to such an extent as to produce concentric cracks.

Therefore it is to expansion of the shell that I feel obliged to look for the force required. There are several ways in which more or less competent radial forces of this nature may be thus brought into play. Some of them are already known to be adequate. About others there is more or less doubt. In the following table I have classified and enumerated all that have come to my notice:

1. Changes of temperature
   a) Sudden heating by fire
   b) Sudden heating by lightning
   c) Seasonal temperature change
   d) Daily or hourly temperature change
2. Expansive force of foreign substances
   a) Frost
   b) Salts
   c) Rootlets of plants
3. Chemical changes
   a) Absorption of water by colloids
   b) Chemical decay of minerals, notably the hydration and oxidation of silicates

CONSIDERATION OF THE FOREGOING FACTORS

1*a*) As to the ability of fire to cause the scaling off of hard rock, there is no room for doubt. In fact, the results of this process have

[1] G. K. Gilbert, "Domes and Dome Structure of the High Sierra," *Bull. Geol. Soc. Amer.*, Vol. XV (1904), pp. 29–36.

been observed by nearly everyone who has ever built a camp fire against a boulder or has seen the results of a city conflagration. The outer part of the rock is so rapidly heated to a high temperature that the expansive force exceeds the tensile strength of the rock. Thin scales, and even slabs one or two inches thick, are thus separated from the solid mass within. Characteristically, the scales are thickest in the middle and taper to a thin edge at the periphery. They are sound and unweathered. Notwithstanding its efficacy, however, this

FIG. 2.—Bowlder of quartzite, cracked by the heat of a forest fire. The curved slabs or spalls may be seen resting on the surface and lying around the base of the block. (Medicine Bow Mountains, Wyoming.)

process does not satisfactorily account for most cases of exfoliating rock, although its effects are prominent in regions that have been ravaged by forest fires (Fig. 2).

1*b*) The effect of lightning, when it strikes rock outcrops, must be essentially the same as that of fire, except that it is far more intense and is localized at a single point. Cases of the shattering of rock by lightning strokes on mountains have been reported, but are too rare to require further attention.

1*c*) In order to disrupt a rock, temperature changes must be more rapid than heat conduction in the mass, and must set up radial stresses that exceed the tensile strength of the rock. Therefore the

two favoring conditions are rapidity of heating and great range in the temperature. Seasonal changes are so gradual that the temperature gradient from the surface inward is a gentle curve. There is no sharp contrast between the highly heated exterior shell and the unaffected interior. It would seem therefore that there should be no great stress developed between the interior and the outer shell.

Nevertheless, N. S. Shaler[1] and several more recent writers on the subject consider seasonal range of temperature effective in producing the sheeting structure commonly noted in granite quarries; and they regard this as only a variety of ordinary exfoliation. T. Nelson Dale,[2] however, has shown that since the sheeting structure is known to extend to depths as great as 250 feet, it is difficult to see how even seasonal changes of temperature—which are believed to be negligible at depths of more than 50 feet—could have produced the observed results.

I have been unable to find any convincing evidence that seasonal insolation is competent to produce either sheeting joints or ordinary exfoliation. Unless the stress is in some degree cumulative, as indicated in the next paragraph, the efficacy of seasonal range in temperature seems to be very doubtful.

1*d*) Daily or hourly changes in temperature, commonly produced by insolation and heat radiation, have long been considered the chief cause of the exfoliation of rock outcrops. More than two-thirds of the authors I have consulted (including all but two authors of textbooks) hold this belief, and most of them do not recognize any other cause. The majority simply state it as an accepted fact, without citing any evidence. Several quote certain explorers to the effect that, in very hot climates, rocks are sometimes split off from cliffs with a sharp report like a gun shot, and this effect is ascribed to temperature changes.

Some writers even go so far as to explain in this way not only the surface exfoliation of blocks, but the splitting of large bowlders right through their centers. I have, however, found no account of

[1] N. S. Shaler, "Notes on the Concentric Structure of Granitic Rocks," *Proceedings Boston Soc. of Nat. History*, Vol. XII (1869), p. 291.

[2] T. Nelson Dale, "The Commercial Granites of New England," *U.S. Geol. Survey Bulletin 738*, pp. 26–38.

anyone having actually observed the process or having proved the theory in any other way. I have not learned of any experimental study of the process, and presume that it still remains to be carried out.

Calculations based on the expansion of a rock surface of given area for certain ranges of temperature are necessarily based on various assumptions regarding the factors involved. The difficulty of ascertaining the values of these basal assumptions largely vitiates such calculations.

Better than estimates are tests made in the laboratory. I have subjected sound igneous rocks to sudden changes of temperature from 15° to 210° C. by plunging the cold rock into boiling oil. In no case did this cause any spalling or cracking or even a visible weakening of the rock. Inasmuch as these laboratory changes were much greater than any possible temperature range in the desert (60°–70° C.), the lack of effect is significant.

Many observers have reported that where a bed of limestone or similar rock has been uncovered in the floor of a quarry, it buckles into low folds. The effect is ascribed to the sun heating the uncovered layer, which had previously been adjusted to a cooler temperature. While the explanation seems entirely reasonable, it should be noted that this is not an instance of the scaling off of a massive rock, but merely of the separation of two layers which were already limited by bedding joints. In such a case the force required is very much less than would be necessary to rend a massive, solid rock.

Much has been written, by J. Walther[1] and others, about the effectiveness of temperature changes in a desert climate in causing the disintegration of ancient monuments in Egypt. However, a new analysis of this problem has recently been published by D. C. Barton,[2] who concludes that temperature change is not the important factor. He finds, for example, that the granite statues in South Egypt, where the moisture is exceedingly low, and rain is almost unknown, are much less affected than those near Cairo, where a small amount of rain falls every year. He also finds that the bases of

[1] J. Walther, *Das Gesetz der Wüstenbildung.*

[2] D. C. Barton, "Notes on the Disintegration of Granite in Egypt," *Journal of Geology*, Vol. XXIV (1916), pp. 382–93.

statues are commonly the only parts exfoliated; and, still more suggestively, he points out that a statue which is situated in a small shaded sanctuary at Karnak and has never been exposed to the sun is rather more damaged in this way than those which receive the full heat of the sun's rays. From this and much other evidence he concludes that the disintegration of these monuments, which has progressed on the average to a depth of about 1 cm. in 5,000 years, is due to the influence of moisture, producing chemical decomposition.

So far as my own field observations go, rocks in very dry deserts are rarely exfoliated. Recently I carefully examined hundreds of bowlders strewn over the great alluvial fans in the Panamint Desert of southeastern California, as well as in the Salton Desert; but I was unable to find any that showed the results of exfoliation. Most of them were sound and unaltered, although rather notably etched and polished by wind abrasion. Yet this is the kind of situation where the effects of insolation are supposed to be greatest. If there is any place with greater daily range of temperature it is possibly on the peaks of high mountains. I have climbed many such peaks, 1,000–3,000 feet above timber-line, and on none of them have I noticed exfoliation. There frost action, particularly along joints and cleavage, is the dominant process, and the result is not scales, but talus and felsenmeer.

Turning from deserts to more humid climates, we find that exfoliation appears abundantly in outcrops of such rocks as granite, gabbro, and diabase, in regions where the diurnal range of temperature is much less than in the desert. Still more significant is the fact that in these moister regions bowlders of exfoliation are found in process of formation five, or even twenty, feet below the surface of the ground. Yet careful measurements have shown that the daily variations of temperature do not penetrate to depths much beyond 10 inches, and even the annual range, at a depth of 25 feet, is less than one-twentieth of what it is at the surface.

From these various considerations I have concluded that much of the exfoliation of rocks cannot be due to temperature changes. I have even grown skeptical regarding the ability of mere insolation to cause exfoliation under any circumstances. If there is convincing evidence that it does so, I have not yet found it.

The only possible escape from this conclusion, so far as I can see, is through the supposition that the strains produced by these changes are to a slight degree cumulative. If it is true that the heated rock never quite returns to its original length after cooling, and therefore that a slight increment of strain is added each time expansion takes place, it is conceivable that after long repetition of the process the strength of the rock might be exceeded, and exfoliation occur. There is at least a hint of this in certain engineering experiments that have been made on the fatigue of metals under repeated strain; but the exact bearing of these upon the behavior of rocks is not apparent.

2a) Frost is known to exert a force capable of rending rocks to pieces. That it can pry apart the cleavage plates of slate, the strata of limestone, the individual grains of sandstone, and probably even the minerals of granite, is a matter of common observation in our colder climates. Whether it is capable of producing the particular phenomenon of exfoliation is not as yet clear. The point of difficulty is to get the water so distributed within the rock as to set up a peripheral stress. After concentric cracks have been formed by another agency it is an easy matter for frost action to widen them. It is well known that water can make its way along mineral cleavage and the sutures between crystals. If this dampening extended into the rock a few millimeters and the moisture then froze, the result would be a peripheral strain such as the case requires, and this stress might well be great enough to rend the rock. At any rate, it seems that frost action is worthy of some consideration, although its value is still problematical. It is possible to investigate this further by experimentation in the laboratory.

2b) In connection with the formation of "desert varnish" we have evidence again that moisture penetrates the outer parts of rock masses to depths of several millimeters, and that, through the aid of such moisture, certain things are dissolved out of the rock and reprecipitated upon the surface. As stated by W. H. Hobbs,[1] it is possible that when such solutions evaporate, minerals, may crystallize in the sutures among the grains and thus bring about an effect essentially the same as that discussed in the preceding paragraph. Like frost action, therefore, the crystallization of salts may be re-

[1] W. H. Hobbs, *Earth Features and Their Meaning* (1912), pp. 201–2.

garded as a possible source of stress, but one about which very little seems to be known.

2*c*) In many places lichens and other small plants grow thickly over the surface of rocks, even in dry situations. They send their rootlets down into any apertures that may be large enough to afford entrance. It is not hard to believe that the growth of such rootlets would be able to pry apart crystals which had already been somewhat loosened by a more competent force. It seems improbable, however, that they could have any effect on fresh igneous rocks, for the pressure of root-growth can hardly be as great as the tensile strength of unweathered rock.

3*a*) It is a familiar fact that many colloids (solid gels) absorb water with avidity, and in so doing swell notably. This effect is readily seen on the outcrops of bentonite. Before the days of powder, quarrymen made use of this principle to crack massive rocks by drilling a row of holes in the rock, inserting closely fitted pegs of wood and then wetting them till they swelled and split the rock. It seems unlikely, however, that this factor is of any importance in connection with the exfoliation of sound igneous rocks, since they do not appear to contain any appreciable amount of colloidal material (Figs. 3 and 4).

3*b*) During the chemical decomposition of rocks, water, carbonic acid, and oxygen enter into combination with the rock minerals, and tend to cause great expansion in volume. The chemical action begins along surfaces and joints and makes its way very slowly into minute cracks and pores in the interior of the joint block. Its progress is visibly marked by the color changes involved, and particularly by the rusting of the iron compounds. Leith and Mead[1] calculate that the increase of volume in the chemical decomposition of a typical granite would amount to about 50 per cent if none of the products of weathering were removed. This increase in the quantity of material is partly offset by the removal of substances in solution during the decay of the mass. In such rocks as limestone, the removal of material in solution greatly overbalances the expansion in volume through hydration and oxidation, and therefore leaves the rock cavernous. On the contrary, when a bed of anhydrite is converted

[1] C. K. Leith and W. J. Mead, *Metamorphic Geology* (1915), p. 92.

into gypsum, there is marked increase of volume due to hydration, and but little solution. In ordinary igneous rocks we have an intermediate condition, in which the hydration of the alumina and iron of the complex silicate minerals causes a greater increase in volume than is offset by the removal of the dissolved alkalies and alkaline earths, and hence the process generally results in a swelling of that part of the rock affected. This expansion is limited to the depths of the shell penetrated by the solutions. Observation shows this to be a matter of millimeters in some cases, of centimeters in others, and of many meters in extreme cases. The expansive force developed

FIG. 3.—A bowlder-like mass of much decayed diabase, showing exfoliation. California Coast Range, near San Luis Obispo. (Diameter 5 inches.)

FIG. 4.—A small exfoliation bowlder of diabase, sawed along the diameter and polished to reveal the concentric cracks which have been produced by expansion of the outer hydrated portion of the rock. Western foothills of the Sierra Nevada, near Copperopolis, California. This bowlder was imbedded three feet deep in residual soil.

is approximately equal to the compressive strength of the crystals of the secondary minerals developed. This force would be insufficient to break very strong rocks by compression; but it is probably more

than adequate to pry them apart by tension, since the tensile strength of all rocks is much lower than their crushing loads.

Although this force of expansion, due chiefly to hydration, has received notice from only a few writers on geology, there are weighty considerations indicating that J. D. Dana[1] was right in regarding it as the chief cause of exfoliation. It is noteworthy that the latter is

FIG. 5.—A mass of diabase in the Coast Range of Southern California, showing the formation of exfoliation bowlders several feet below the surface of the ground.

most prominent in rocks consisting largely of feldspar, olivine, augite, and other complex silicates. On the other hand, I have not observed it in quartzites, limestones, and other rocks of simple and unfavorable mineral composition. It is very significant that exfoliation is especially common in warm, rather moist climates, such as the west slope of the Sierra Nevada of California, southeastern Brazil, southeastern China, etc. Excellent examples are to be found near San Luis Obispo on the southern coast of California (Fig. 5), and in the celebrated Stone Mountain of Georgia. All of these are regions

[1] J. D. Dana, *Manual of Geology*, 4th ed. (1896), p. 127.

where temperature changes are less than in deserts and on alpine peaks, but where hydration and oxidation are distinctly favored.

The most cogent evidence in support of the opinion that exfoliation is due to chemical decay is, in my estimation, the common development of concentric shells in jointed granitoid rocks at depths of

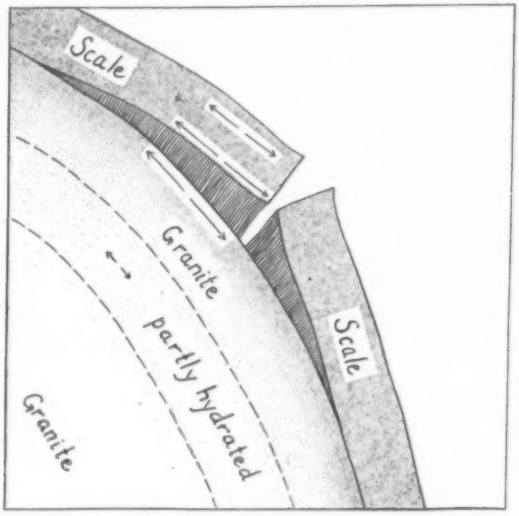

FIG. 6.—Polygonal cracks on the surface of exfoliating bowlders. The diagram represents a cross-section of part of a bowlder of granite. It shows, from left to right, the unaltered granite, then the progressively more and more hydrated granite in which exfoliation cracks are developing, and finally the outermost shell which has cracked and begun to separate from the inner portion. The arrows indicate the direction of expansion due to hydration, and the varying lengths of the arrows indicate roughly the relative intensity of the expansive force. It will be noted that the latter is greater on the under side of the outermost scale than on the exterior. The shaded portion is always in shadow, and retains moisture longer than the outside.

10–20 feet below the turf or forest cover in moist regions. In such places one can trace the gradation between the typical residual soil and the exfoliating bowlders imbedded in it.

In dry regions I have often seen on the surface of exfoliating bowlders rudely polygonal cracks delimiting scales several inches in breadth (Fig. 6). The scales are slightly tangent to the curved surface

of the bowlder, suggesting to some degree the concave plates in sun-dried mud. Cases which appear to be somewhat similar have been described from the coastal desert of Peru by C. B. Brown,[1] who regards them as the result of intense heating by the sun, but does not explain just how the process works. The best explanation which has occurred to me is this: During the process of exfoliation of such an exposed bowlder the concentric cracks serve as channels for the slow percolation of water, whenever there is rain. During the dry season the sun quickly desiccates the outer side of the plate, but the moisture remains on the shady underside for a much longer time, and there continues its chemical work. On this account, the hydration of the minerals on both walls of the concentric crack proceeds more rapidly than it does on the dry exterior of the slab. The somewhat greater expansion of the under surface, thus induced, warps the plate in the manner described. It is significant that I have never been able to find this phenomenon shown in a new quarry or road-cut below the surface, but only upon natural outcrops or bowlders, and chiefly in rather dry situations. The best examples I have seen were in Owens Valley, California.

SUMMARY

Rocks, especially the feldspathic kinds, often weather into concentric shells and round bowlder-like masses. The process is ascribed by nearly all textbook writers to temperature changes, and especially to those from day to night. Other causes which have been suggested are shrinkage on cooling, crystallization of ice or salts in pores, and katamorphic alteration. It is generally stated that the process is particularly effective in deserts and on high mountain tops.

On the contrary, it seems to be a fact that exfoliation develops best in moist situations. The process goes on not merely at the surface, but at depths of several feet, beneath thick residual soil. Experiments indicate that change in temperature, unless it involves high temperatures, such as those in fires, is powerless to split rocks, although there remains the possibility that such strains may be gradually accumulated until rupture occurs. On the other hand,

[1] C. B. Brown, "On Some Effects of Wind and Sun in the Desert of Tumbez, Peru," *Geological Magazine*, Vol. LXI (1924), pp. 337–39.

there is convincing evidence that katamorphism, and particularly hydration, is the chief cause of the peripheral expansion which results in the formation of the concentric shells.

Exfoliation, indeed, appears to be at least two quite distinct processes under a single name. They may be called (*a*) spalling and (*b*) chemical exfoliation.

Spalling is known to be caused by fire, but in nature that agency is rarely brought into play, except in forested regions. It is said to be caused also by solar heating, but the ability of this process is open to serious doubts. True spalling is particularly effective in dense brittle rocks, such as quartzite, glass, and fine-grained igneous rocks. The fragments produced are generally undecayed.

Another possible cause of spalling is frost action; and the wedgework of salts and possibly of roots may be factors of consequence. In general, however, these activities produce a loose rubble of angular, undecayed crystals, rather than well-defined slabs or sheets.

In chemical exfoliation the sheets are notably decayed and discolored. The work is performed by solutions which penetrate slowly along the cleavage cracks of crystals and between the grains, and there induce the formation of new minerals of larger volume.

CORRELATION OF PERMO-CARBONIFEROUS RED BEDS IN SOUTHWESTERN COLORADO AND NORTHERN NEW MEXICO[1]

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ABSTRACT

Two summers of field work in the Sangre de Cristo Range of Colorado, together with a critical examination of the geologic literature on the more important districts immediately to the north, west, and south of this range, have brought the writer to certain new conclusions regarding the correlation of the Permo-Pennsylvanian red beds of this region. On account of the complexity of the nomenclature, it is necessary to give a short summary of previous correlations. A simplification of the nomenclature is subsequently proposed; and finally, the writer's suggested correlation is presented, together with evidence for the changes.

INTRODUCTION

The most important districts considered in this correlation are the San Juan Mountains, the Crested Butte district, the Leadville-Tenmile district, the Sangre de Cristo Range north of La Veta Pass in Colorado, and the southern end of the Sangre de Cristo Range between Las Vegas and Santa Fe, New Mexico. It will be observed that these separate districts, with the exception of the Leadville-Tenmile district, are ranged about the large area occupied by the San Luis and upper Rio Grande valleys, La Garita Hills, Cochetopa Hills, Vernal Mesa, and Uncompahgre Plateau. In an earlier paper the writer has called attention to this central area as the probable position of high mountains which yielded an abundance of sediments for the red beds of late Pennsylvanian and Permian time.[2] The Aspen district and the Front Range are also considered, but their greater distance from this central "positive" area makes correlation in their case more uncertain.

[1] It is a pleasure to acknowledge the writer's indebtedness to Professor R. T. Chamberlin, of the University of Chicago, for suggestions leading to the writer's studies in the Sangre de Cristo range of Colorado, and to Professor P. G. Worcester, of Colorado University, for specific suggestions relative to the place of origin of the Permo-Carboniferous red beds of southwestern Colorado.

[2] F. A. Melton, "The Ancestral Rocky Mountains of Colorado and New Mexico," *Jour. Geol.*, Vol. XXXIII (1925), pp. 84–89.

HISTORY OF CORRELATION

The first writer to examine the larger characteristics of these Carboniferous red beds in southwestern Colorado and to attempt a correlation was G. H. Girty.[1] His results were quite consistent with the facts then known, and the general equivalency of formations established by him has not, to the writer's knowledge, thus far been questioned. He has, however, somewhat modified his earlier opinions regarding the position of the Pennsylvanian-Permian boundary as additional evidence has been discovered.[2]

In 1903 Girty correlated the Hermosa formation (of Pennsylvanian age) in southwestern Colorado[3] with the Lower Maroon conglomerate and the Weber formation of the Crested Butte district[4] and the Weber grits of the Leadville-Tenmile district.[5] In 1909[6] he also provisionally correlated the Hermosa formation with the Magdalena limestone group of New Mexico. Later he came to regard it as the probable equivalent of the Fountain and Lyons formations of the Front Range as well.[7]

The correlation of the Rico formation of the San Juan area with the "Upper Maroon conglomerate" of the Crested Butte district, with the upper part of the "Maroon conglomerate" of the Aspen district,[8] and with the "Maroon formation" of the Leadville-Tenmile district (Fig. 1) was mentioned by Girty, in 1903, with considerable reserve.[9] He pointed out that fossil evidence for such a correlation was practically nonexistent, but apparently considered that the lithology and stratigraphy suggested it. Also, in 1909, the

[1] G. H. Girty, *U.S. Geol. Survey Prof. Paper 16*, 1903.

[2] G. H. Girty and W. T. Lee, *U.S. Geol. Survey Bull. 389*, 1909; and G. H. Girty, *Annals New York Acad. Science*, Vol. XXII, 1912.

[3] See various geologic folios of the San Juan area in southwestern Colorado. The Engineer Mountain Quadrangle is considered typical.

[4] Emmons, Cross, and Eldridge, *Anthracite-Crested Butte Folio*.

[5] S. F. Emmons, *U.S. Geol. Survey Monograph No. 12*, 1886, and *U.S. Geol. Survey Folio No. 48*, 1898.

[6] G. H. Girty and W. T. Lee, *U.S. Geol. Survey Bull. 389* (1909), p. 43.

[7] G. H. Girty, Various folios of western and central Colorado; and G. B. Richardson, *Castle Rock Folio* (Colorado, 1915), p. 5.

[8] J. E. Spurr, *U.S. Geol. Survey Monograph No.* 31, 1898.

[9] *U.S. Geol. Survey Prof. Paper* 16 (1903), pp. 262–63.

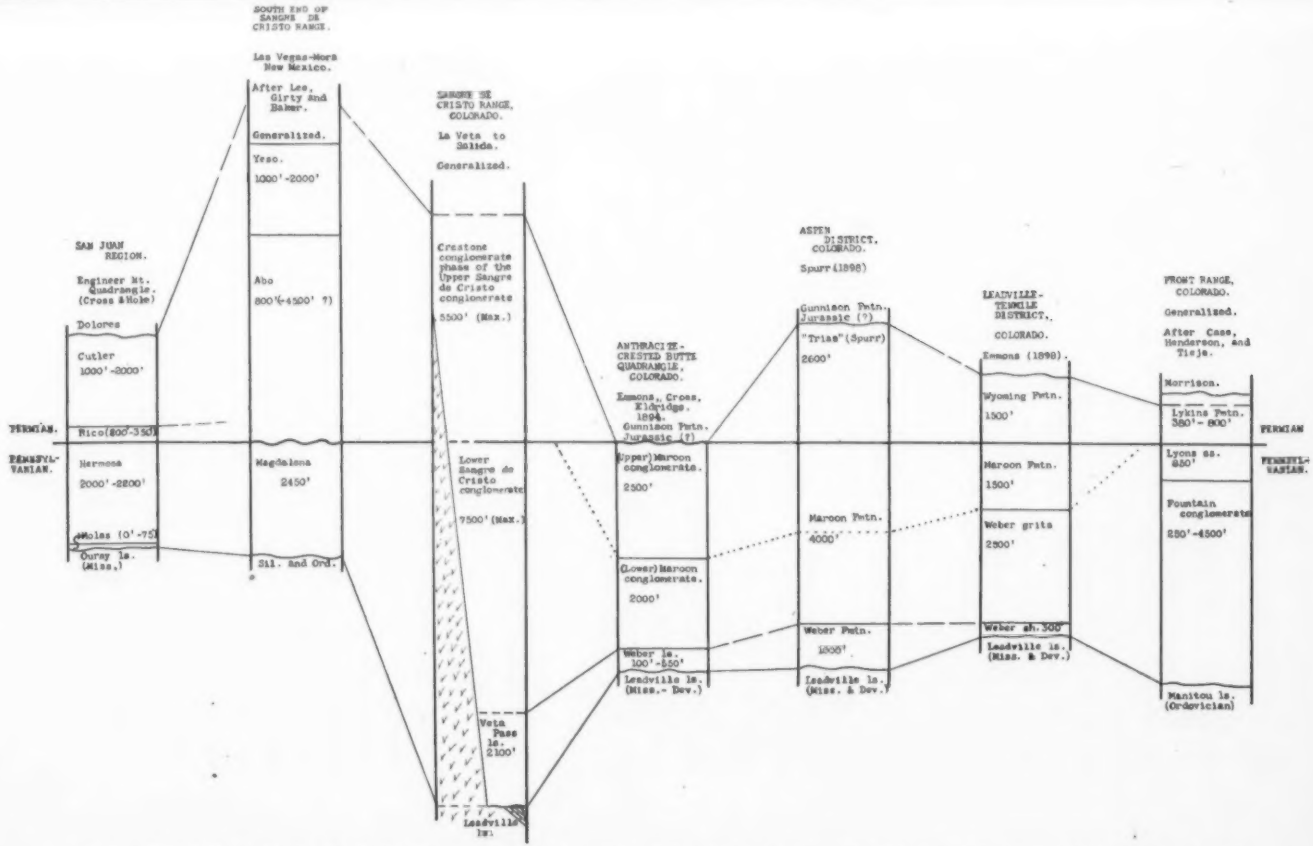

FIG. 1.—Correlation table for the Carboniferous red beds of southern Colorado and northern New Mexico. Maximum thicknesses are platted

equivalency of the Rico formation and the Manzano group of New Mexico was strongly suggested by Girty[1] on lithologic, stratigraphic, and faunal grounds. Later, in 1912,[2] he tentatively correlated the Rico formation with the lower part of the Lykins formation of the Front Range on fossil evidence, and, apparently for the first time, placed the Rico reservedly in the Permian. Since then he has evidently become more and more convinced that the base of the Rico should be the Pennsylvanian-Permian boundary in southwestern Colorado.

All writers seem to agree in placing the unfossiliferous bright-red series above the Rico (the Cutler formation of the San Juan area) rather hesitatingly in the Permian. In this area it is overlain unconformably by the Dolores formation of Triassic age and, if the Rico is correctly referred to the Permian, it seems that the Cutler must be Permian also. Farther west, in the Dolores River region, this unconformity seems to play out, and the Cutler and Dolores, both being bright-red sandstones and shales, are in some places separated only with difficulty. Butters[3] believes that the greater part of the Lykins formation of the Front Range (i.e., the part above the crinkled sandstone—except the extreme upper portion in Larimer County) should be correlated with the Cutler.

PROPOSED SIMPLIFICATION OF NOMENCLATURE

The usage of formational names is so confusing in central Colorado that throughout the following discussion an attempt will be made to simplify it. Since the sections in the Crested Butte, Aspen, and Leadville-Tenmile districts are quite similar, it is proposed to call the fossiliferous limestone and shale horizon, at the bottom in the three areas, the Weber formation. The series of conglomerates, arkoses, sandstones, and shales which are above the Weber formation and below the bright-red series at the top of the sections, and which are from 4,000 to 4,500 feet thick, will be called, uniformly, the Maroon conglomerate. Upper and Lower divisions of the Maroon conglomerate will be recognized in all but one of the three

[1] G. H. Girty and W. T. Lee, *U.S. Geol. Survey Bull. 389* (1909), p. 42.

[2] G. H. Girty, *Annals New York Acad. Science*, Vol. XXII (1912), pp. 1–8.

[3] R. M. Butters, *Colorado Geol. Survey Bull. 5*, Part II (1913), p. 86.

districts—the Aspen district—as lithology suggests and as usage has already sanctioned. Application of the two terms, Upper and Lower Maroon conglomerate, even to the succession in the Aspen district, seems to be favored by Spurr's description of formations,[1] but the twofold division was not emphasized by him, and it is thought best to leave it out of this discussion.

The terms Upper and Lower Maroon conglomerate will be used here just as they have been applied in the Crested Butte district, and also for the terms Maroon formation and Weber grits, respectively, as they were applied by Emmons (1898) to the Leadville-Tenmile district.

SANGRE DE CRISTO CONGLOMERATE

In the Sangre de Cristo Range of southern Colorado there exists a succession of Permo-Pennsylvanian red beds so similar to the succession in the separate districts to the north and west, just mentioned, that a general correlation is readily made. The entire series of Permo-Pennsylvanian red beds in this range has been named, by R. C. Hills,[2] the Sangre de Cristo conglomerate.

Considered broadly, this formation can be said to consist of conglomerates, arkoses, and shales, and is noteworthy for the red color that is present in various shades throughout almost the entire formation. The formation's most characteristic development, as well as its greatest thickness, is reached in the central part of the Sangre de Cristo Range near the village of Crestone, Colorado, where it is probably at least 13,000 feet thick, neither the upper nor the lower contact being exposed where estimates of thickness were made, and where it forms the backbone of the range. From this locality it thins irregularly in both directions along the range until it amounts to but 6,000 feet at one place in the Culebra Range near the southern border of the state. North of Crestone it again thins to 4,300 feet in the valley of the Arkansas River southeast of Salida.

Near Crestone, where the formation has its greatest development, it is easily divided into two groups of strata—the Upper

[1] J. E. Spurr, *U.S. Geol. Survey Monograph No. 31*, 1898.

[2] R. C. Hills, *U.S. Geol. Survey Folio No. 58* (Elmoro), 1899.

Sangre de Cristo conglomerate, consisting of very coarse, red conglomerates estimated to be about 5,500 feet thick, and the Lower Sangre de Cristo conglomerate, which is composed of finer conglomerates and arkoses, darker red in color, and about 7,500 feet in thickness.

A series of black limestones and shales, containing marine fossils of Pennsylvanian age, is present in the bottom part of the formation, interbedded with the coarse arkoses and micaceous shales of continental origin. So far as known, its maximum thickness, 2,100 feet, is found at Placer, near La Veta Pass, Colorado, and from this exposure it has been named by the writer the Veta Pass limestone member of the Lower Sangre de Cristo conglomerate.

The largest bowlders in the Upper Sangre de Cristo conglomerate have a diameter of about 8 feet, but this extreme coarseness, even though it is a very prominent feature near Crestone, is, nevertheless, only of local development, and in both directions along the range the upper series becomes thinner and finer in grain. For this reason the coarse conglomeratic series has been named by the writer the Crestone conglomerate *phase* of the Upper Sangre de Cristo conglomerate.

Near Crestone the coarse phase overlaps the Lower conglomerate and rests directly on Pre-Cambrian granite. Though an angular unconformity was not seen between the two divisions of the Sangre de Cristo conglomerate, it is believed that further search in this area may reveal such a break.

SUGGESTED CORRELATION

Bearing in mind the various evidences for believing that the large area of the San Luis and upper Rio Grande valleys, and the region farther to the northwest, was the source of supply for a large part of the Permo-Pennsylvanian red beds, the correlation advanced below becomes quite understandable. These evidences, in brief, are: first, the presence in this region of a large area of pre-Cambrian rocks directly overlain by the Jurassic, La Plata sandstone, and also by Tertiary extrusive rocks; second, the diminution in coarseness of the Permian beds under consideration, as they are traced away from the area affording the sediments; and third, the overlapping

of older formations by the Permian conglomerates near Gateway, Colorado, southwest of the Uncompahgre Plateau, and again in the Sangre de Cristo Range south of Crestone, Colorado. The lack of a Permian series in the Crested Butte district is no doubt due to beveling of the Caboniferous strata by erosion in pre-Jurassic time. With the place of origin of these sediments thus determined, the correlation of the coarse, bright-red, Crestone conglomerate of the Sangre de Cristo Range, with the coarse, bright-red Cutler formation of the San Juan area, and with the similar Wyoming formation of the Leadville-Tennile district is strongly suggested.

The prominent lithologic change at the bottom of these coarse red beds in southwestern Colorado, as well as the probable stratigraphic break below the coarse phase in the Sangre de Cristo Range, and the possible break below the Cutler formation, southwest of the Uncompahgre Plateau near Gateway, are all, in the opinion of the writer, the equivalent of the stratigraphic break beneath the Rico formation[1] in the San Juan area, and also the equivalent of the unconformity between the Magdalena and Manzano groups of north-central New Mexico, which Girty and Lee believe should mark the Pennsylvanian-Permian boundary.[2]

This correlation of the coarse, bright-red Permian formations makes it necessary to place in the Pennsylvanian the varicolored arkoses, sandstones, and shales which occur below. The Pennsylvanian age of the lowest part of the series, the Weber formation, and the Veta Pass limestone, is established by the marine invertebrate fossils that these limestones and shales contain. Accordingly,

[1] Mentioned as a strong probability by Girty in *Prof. Paper No. 16* (1903), pp. 257–58.

[2] Böse (*Amer. Jour. Science*, 4th series, Vol. XLIX [1920], pp. 51–60), on the basis of fossil evidence from the lower part of the Abo formation (basal Manzano), calls it Upper Pennsylvanian (Uralian) in age. Baker (*op. cit.*, pp. 99–126) says: "The lower part of the Abo formation is Upper Pennsylvanian in age." Lee and Girty believe the base of the Permian should be placed at the unconformity between the Abo formation and the underlying Magdalena group. The view that this unconformity appropriately constitutes the division between Pennsylvanian and Permian time has been the official usage of the U.S. Geol. Survey for several years. Lee apparently believes this unconformity, at the base of the Manzano group in New Mexico, is also known in Oklahoma, west Texas, Arizona, Utah, and much of Colorado and Wyoming, and that it is too prominent a stratigraphic break to be superseded by the conflicting fossil evidence now available.

the writer correlates the Lower Sangre de Cristo conglomerate with the Hermosa formation of the San Juan region; with the Weber formation, the Lower Maroon conglomerate, and also the *Upper Maroon conglomerate* of the Crested Butte, Aspen, and Leadville-Tenmile districts; with the Magdalena group of New Mexico, according to previous correlation of the Hermosa formation by Girty, and with the Fountain conglomerate and Lyons sandstone of the Front Range (Fig. 1).

The only departure from previous usage here is the addition to the above groups of correlated strata, of the *Upper Maroon conglomerate* of the Crested Butte, Leadville-Tenmile (and Aspen) districts. Girty, in *Professional Paper No. 16*, 1903, being apparently moved by the similar maroon color of the Rico formation and the upper part of the Maroon conglomerate, suggested their correlation, even though the Rico is only 350 feet thick and the Upper Maroon reaches 2,500 feet near Crested Butte. He pointed out plainly, however, that fossil evidence was almost totally lacking, and made it clear that this proposed correlation could be only a suggestion in the light of the existing evidence.

The dotted line in Figure 1 indicates where the Permian-Pennsylvanian contact would probably be drawn following previous publications, and the formations above it represent the part that is here put in the Pennsylvanian.

It is clear that an attempt to correlate the Rico formation with the maroon-colored beds elsewhere involves drawing the Pennsylvanian-Permian contact within an unbroken series of strata (at the bottom of the Upper Maroon conglomerate), and not at the top of the Upper Maroon conglomerate and the base of the overlying coarse, bright-red conglomerates, as this paper advocates.

The writer believes that the correlation herein proposed is strong evidence against using the maroon color as a basis for correlation. Maroon-colored arkoses, sandstones, and shales are quite prominent in the middle one-third of most of the sections under consideration; nevertheless the color shows such pronounced variations from place to place that a color correlation would surely lead to error.

For example, north of Hillside, in the Wet Mountain Valley,

the maroon color characterizes the entire Lower Sangre de Cristo conglomerate, and in the valley of the Arkansas River, below Salida, the entire thickness of the Carboniferous series shows the dark-red color. The lower part of the Maroon conglomerate near Aspen is chocolate-red in color, while its probable equivalents—the Lower Maroon of the Crested Butte and Leadville-Tenmile districts—are described as yellowish-gray and "white," respectively. Though the Hermosa formation of the San Juan region is characteristically gray, greenish, or buff in color, near Ouray, Colorado, pink and red horizons are present in the upper and greater part.

While it appears that the lower limit of the maroon color may vary widely from place to place, the same does not seem to be true of the lower limit of the bright-red color of most of the described sections. As already pointed out, the brighter color seems to begin, in southwestern Colorado, with the prominent change in depositional history that is thought to mark the beginning of Permian time.

AMPHITHEATER VALLEY HEADS

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In an article recently published in the *Journal of Geology*, Freeman[1] described the topographic characters and mode of origin of certain valley heads in an anticlinal mountain range in central Montana; these valley heads, because of their similarity to glacial cirques, were called "pseudo-cirques." The series of rocks present in the range consists of 2,300 feet of limestone, underlain, in order of position, by 700 feet of shale, 80–100 feet of quartzite conglomerate, and 200 feet of shale. Since no evidences of glaciation have been observed anywhere in the range, the origin of the amphitheater-like valley heads was ascribed to the undermining of the relatively easily eroded shales, and the continuous steepening of the resistant limestone faces by the collapse of unsupported masses of that rock. Freeman considered that certain structural conditions are specially favorable for the production of these great amphitheater valley heads: (1) an anticlinal fold with nearly flat, thick, resistant strata at the top or along the axis of the anticline, and steeply dipping strata along the limbs; (2) a sufficient area of flat-lying beds to permit the development of the amphitheaters; and (3) soft, easily eroded strata underlying the resistant beds.

In regions of flat-lying or gently dipping strata, composed of thick, resistant members underlain by thinner beds of weak rocks, the development of amphitheater valley heads takes place if the streams have cut through a resistant layer into the weaker rocks below. The perfection of form and the size of the amphitheaters depend upon various conditions. Of prime importance are the thickness, the number, and the resistance of the harder strata. If more than one thick, resistant bed, separated by weaker layers, be

[1] O. W. Freeman, "The Origin of Swimming Woman Canyon, Big Snowy Mountains, Montana, an Example of a Pseudo-Cirque Formed by Landslide Sapping," *Journal of Geology*, Vol. XXIII (1925), pp. 75–79.

present and be eroded through, the amphitheaters will tend to have a nested structure. In the case cited by Freeman, the 2,300 feet of limestone are practically unbroken by softer beds; hence but a single cliff is present at the heads of the valleys. As with glacial cirques, the amphitheaters may be simple, or varying numbers may coalesce into compound forms.

As is well known, magnificent amphitheaters form the heads of many of the valleys tributary to the Colorado River in the Grand

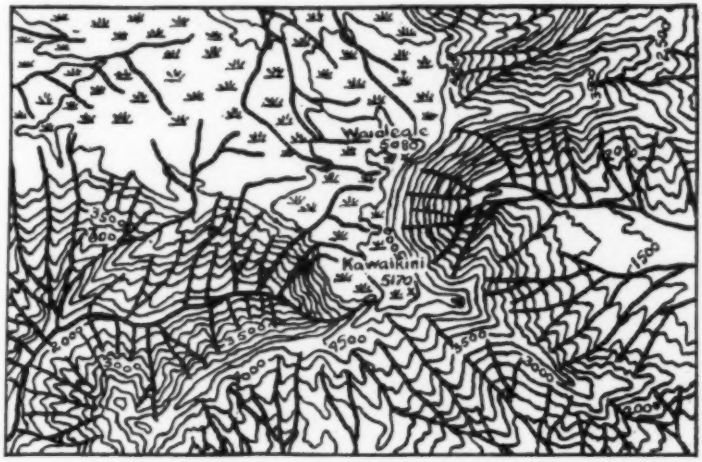

FIG. 1.—Ampitheater valley heads eroded into an extinct lava dome. Redrawn from U.S. Geological Survey topographic map of Kauai County, Hawaii. Contour interval 250 feet.

and other canyons cut in rock structures of the type indicated above. In the Grand Canyon the alternation of a number of thick, resistant limestones and sandstones with shales is responsible for the nested character of the amphitheaters above the Granite Gorge. Into many extinct lava domes, which are built of flows dipping at low angles from more or less centrally located principal eruptive areas, streams have cut deep canyons which head in huge, almost vertically walled amphitheaters (Fig. 1). The lavas of the domes vary somewhat in their resistance to weathering and erosion; consequently there is more or less continuous sapping along the weaker members, and

slumping of masses of rock from above. The presence of weaker flows at various horizons results in the production of a modified cliff-bench topography in the canyon walls, and occasionally of poorly developed nested amphitheaters.

In short, the amphitheater valley head is a common feature of normal river erosion, and is present in a much wider range of rock structures than Freeman indicates; hence this form should not receive a name which has been restricted to glacially modified valley heads, land forms of secondary origin. Also, the prefix "pseudo" suggests that these amphitheaters are rare, or at least uncommon, which certainly is not the case. The use of the term "pseudo-cirque" will result in a confusion of ideas; the name "amphitheater head" is equally applicable and does not trespass upon the nomenclature of glaciation.

A CRITICISM OF THE APPLICATION OF THE THEORY OF ASSIMILATION TO THE SUDBURY SHEET[1]

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ABSTRACT

In this paper the article by Mr. G. W. Bain on the "Amount of Assimilation by the Sudbury Norite Sheet" is criticized and certain fallacies in the argument are pointed out. A comparison is made with the Arnage norite of Aberdeenshire, which provides an undoubted case of assimilation by a gabbroic rock.

During the summer of 1924 the writer conducted a detailed study of the rocks of the Sudbury district. The results of that work are now in process of publication by the Ontario Department of Mines. It was, therefore, with considerable interest that the writer read Mr. G. W. Bain's argument for assimilation in the Sudbury sheet presented in the July-August number of the *Journal*.

At the outset it seems that Mr. Bain does not give sufficient ground for his cleverly worked out consideration of the assimilation hypothesis as applied to the Sudbury sheet. He refers to the work of geologists who, having observed xenoliths of country rock partly digested in an igneous rock, set themselves the problem of determining how much of the country rock had been wholly assimilated without leaving the slightest trace in the way of ghost-like remnants. In their case the problem was certainly justifiable, and was undoubtedly indicated in the field data. But what evidence does Mr. Bain adduce, of rock full of xenoliths passing through less and less contaminated material into pure gabbroic rock, that he can by calculation alone reach the conclusion that "the norite magma has assimilated an amount of rock equivalent to two-thirds of its original volume, and possibly as much as two to three times that amount"? If assimilation had been so pronounced, surely we should have expected to find conclusive field evidence of the process, such as is to be seen in the case of the contaminated norite of the Arnage mass,

[1] Published with the permission of the Department of Mines, Ontario.

Aberdeenshire, described by Dr. Read.[1] In this latter instance "there is a gradual passage from country-rock to contaminated rock, and from this to norite," the contaminated rock showing partly digested xenoliths, which are often sufficiently abundant to impart to weathered surfaces of the rock a conglomeratic aspect.[2] The writer saw nothing even approaching this type of assimilation in the Sudbury sheet. Professor Coleman, speaking of the possibility of assimilation having taken place, states that he saw "no direct evidence of stoping on a large scale from the overlying conglomerate."[3] The evidence to which he refers is the presence of partially digested blocks of the conglomerate in the micropegmatite. It must be remembered, however, that Professor Coleman considered that assimilation might have taken place. The evidence on which he bases his conclusion is the apparent transition from micropegmatite to Trout Lake Conglomerate. But to the writer it seems that this phenomenon can be explained without having recourse to the theory of assimilation. Professor Coleman himself gives the explanation in an attempt to harmonize the absence of partially digested inclusions of conglomerate in the micropegmatite with the assimilation theory. He states, ". . . . later the eruptive became too viscid to allow blocks to sink, but worked on the overlying rock by heated vapors or solutions, causing the remarkable blending of the eruptive with the conglomerate."[4] But if this apparent gradation between micropegmatite and conglomerate is to be explained by the action of these heated vapors from the former, wherein lies the necessity for assuming, without further evidence, that assimilation has gone on? The apparent transition from micropegmatite to Trout Lake Conglomerate depends, in the writer's opinion, more on the similarity in the field of the micropegmatite and the highly contact-metamorphosed sediment than it does on the mineralogical character of these two rocks. The conglomerate next the micropegmatite retains, in almost every case, some trace of its original clastic texture, and by making use of this criterion the contact of the two rocks can be located to within a few feet, where there is no drift.

[1] H. H. Read, *Quart. Jour. Geol. Soc.*, Vol. LXXIX (1923), pp. 446–86.

[2] *Op. cit.*, pp. 458–59. [3] A. P. Coleman, *Jour. of Geol.*, Vol. XV (1907), p. 773.

[4] A. P. Coleman, *op. cit.*, p. 774.

Another objection to postulating assimilation in the case of the Sudbury sheet lies in the absence of any abnormal rock type. In the case of the Arnage mass we find that the contaminated rock carries cordierite, garnet, spinel, and tourmaline.[1] Here again there is no similarity with the Sudbury sheet. In this matter, then, as in the case of absence of xenoliths and the character of the contact of the micropegmatite, there seems no valid reason for complicating the problem by bringing in a theory of assimilation.

Even if we accept the possibility that assimilation has taken place, there are one or two other points in Mr. Bain's argument which need further discussion. In the first place, it is disappointing to note that the analyses which Mr. Bain has made personally are not from the Sudbury "norite,"[2] but from what he terms "its prolongation north of the Spanish River." In the writer's opinion it is not at all certain that this rock is the Sudbury "norite." The Kelly Lake gabbro is near to the Sudbury sheet and resembles it in some respects, but is yet considered to be a little older than the latter.[3] It is evident in any case, however, that if an important conclusion is to be drawn as to the genesis of the Sudbury "norite" and micropegmatite, the data are best collected from these rocks themselves, and not from one removed 10 or 15 miles from them.

With regard to the interpretation of the composition of the medium-grained marginal phase of the "norite," Mr. Bain does not appear to give sufficient weight to the action of the late silicic differentiates in the magma. The writer's observations, both in the field and the laboratory, show clearly that there was a late differentiation of quartz to the earlier cooled basal portion of the sheet. Blue quartz occurs as small veinlets, and also corrodes and cuts across the first-formed minerals. Professor Colony has already emphasized the action of these late silicic differentiates in a recent paper.[4] The marginal facies of the "norite," therefore, can be taken as represent-

[1] H. H. Read, *op. cit.*, p. 458.

[2] Quotation marks are inserted here, since the writer considers that the rock is more correctly termed a gabbro.

[3] See in this connection C. W. Knight, *Report Royal Ontario Nickel Commission* (1917), p. 108.

[4] R. J. Colony, *Jour. of Geol.*, Vol. XXI (1923), pp. 169–78.

ing the composition of the original magma only in a very general way, and cannot serve as the basis of an exact calculation such as Mr. Bain employs.

A third point on which the writer disagrees with Mr. Bain is in the fitness of the graphs he has drawn to provide a basis for so precise a calculation as he later employs. The figures are not on one line of section, but are collected from isolated samples from the southern range and from the rock north of Spanish River. Furthermore, reference to the curves shows how far separated are many of the points, and therefore how doubtful is the slope of the main portions of the curves. And yet it is from such graphs that Mr. Bain makes the integration of (percentage composition) – (distance) from which he obtains his averages! The error involved here might well cause an error in his figures greater than the differences listed in the tables. When he plots the analyses given by Mr. Knight of the igneous rock in MacLennan Township, the chance of error in his very general graphs becomes evident. One is not justified in drawing a continuous curve in such cases. The only satisfactory method is to join up the points by straight lines so that no assumptions are made as to the laws of the variation. When this is done with the figures given by Mr. Knight, the interpretation is somewhat different from that assumed by Mr. Bain. The very sharp break in the graph indicates two separate rocks, as suggested by Dr. Harker, and this is a possibility which Mr. Bain has evidently not considered.

A fourth point open to criticism is the statement made by Mr. Bain that no appreciable amount of material has been subtracted from the magma. This is the *sine qua non* of his discussion, for it provides his criterion for determining which of the oxides has remained constant throughout the assimilation process. He arrives at the conclusion that no material has passed from the magma, by a consideration of the contact effects. These contact effects he holds are never greater than 100–500 feet. In the case of the contact of the micropegmatite and the Trout Lake Conglomerate these figures are much too low, for the conglomerate is markedly altered for at least 0.25 miles from the contact, the micropegmatite having been evidently very rich in late magmatic waters. It is, therefore, against the field evidence to hold that no material has passed from the

magma into the country rock. The error involved here runs through all of Mr. Bain's calculations. In considering the difference between "actual and anticipated percentages," he assumes that it must be the MgO which has remained constant, whereas if one regarded the analyses apart from the hypothesis that nothing had passed from the magma, the Fe_2O_3 would be considered as the constant.

As a further step in his argument, he assumes that the parent magma has *gained* MgO. No reason is given for this assumption beyond a reference to MgO in a quartzite which later he brings in as a possible source of contamination of the parent magma. Now, if we refer to the account of the Arnage norite and the comparison made there with other contaminated norites, we see that, "compared with the initial magma, the contaminated rock is *poorer* in lime and magnesia, while, compared with the initial sediment, the xenolith is richer in these two oxides."[1] Let it be remembered here also that the original rock of these xenoliths in the Arnage norite contains up to 4.33 per cent of MgO, while the Mississagi quartzite analyzed by Mr. Bain contains only 0.46 per cent.[2] When he considers Mr. Knight's analyses, it is the Al_2O_3 which he has to consider constant, while under ordinary circumstances one would take the FeO as the constant element in the transfer. He holds that between his calculations for the first series of curves and for these based on Mr. Knight's analyses there is little difference, and yet the Fe_2O_3, which, under normal circumstances, would be considered as the constant element in the first instance, shows a percentage change of +175.50 per cent in the second. This is a feature of which he takes little note.

Apart from the data and calculations put forward by Mr. Bain there are two features of the conclusion drawn which he does not sufficiently consider. In the first place, it is by no means certain that a gabbro rock can assimilate an acid sediment. If reference be made to one of the early papers on assimilation, that by H. J. Johnston-Lavis and J. W. Gregory on the inclusions found at Monte Somma, it will be noted that the little quartz present in the older dolerites through which the magma has passed has been ejected with the other minerals, and not assimilated by the uprising molten

[1] H. H. Read, *op. cit.*, pp. 481–83.

[2] G. W. Bain, *Jour. of Geol.*, Vol. XXXIII (1925), p. 517.

rock.[1] This refractability of siliceous material to assimilation by a basic magma is further emphasized by these writers at the conclusion of their paper.[2] In the case of the Arnage rock, Dr. Read shows that the quartzite country rock has actually been rejected in the contamination process.[3] In the face of such evidence, then, based on observations of undoubted cases of assimilation, it does not seem reasonable to assume, on speculative grounds alone, that extensive assimilation of quartzite has gone on in the case of the Sudbury sheet. In the second place, even allowing for the action of water, the heat required to melt sediment to the amount of two to three times the bulk of the Sudbury "norite" would be tremendous. If we are going to allow such enormous melting power to the Sudbury gabbro intrusion, then we are confronted with the difficulty of explaining away the absence of this power in the overwhelming majority of intrusions into the earth's crust.

[1] H. J. Johnston-Lavis and J. W. Gregory, *Sci. Trans. Roy. Dubl. Soc.*, Ser. 2, Vol. V (1893–96), p. 265.

[2] *Op. cit.*, p. 277.

[3] H. H. Read, *op cit.*, p. 464.

THE AMOUNT OF ASSIMILATION BY THE SUDBURY NORITE SHEET

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In an interesting paper on the "Amount of Assimilation by the Sudbury Norite Sheet" G. W. Bain[1] calls attention to certain features of that much-studied igneous mass that are of particular importance in connection with the genesis of the various rock types. Bain points to the existence at the base of the mass of a contact phase of finer grain which he interprets as the rapidly chilled original magma. Immediately overlying this marginal phase there extends upward for several hundred feet a variety of rocks that are definitely more basic and these in turn pass upward into the well-known micropegmatite facies of the sheet.

The arrangement described by Bain is thus very similar to that in the Palisade sheet with its somewhat more basic layer lying above the chilled basal facies and passing upward into the more acidic types. Bain's conclusion that the fine-grained material at the base represents the original magma would appear to be entirely justified, especially in the light of the overlying layers that are definitely more basic. If there was no addition of material to the sheet after the consolidation of the chilled base, the average composition of the overlying mass and, of course, of the whole mass, should be sensibly that of the basal layer, an assumption which, again, appears to be entirely justified. With these considerations in mind Bain then proceeds to calculate the average composition of the mass from the chemical composition of analyzed types, assigning due weight to each type by a suitable graphical method, and finds an average composition much more salic than that of the basal chilled phase. He concludes, therefore, that subsequent to the consolidation of that phase, salic material must have been added to the mass, and here again we must find him entirely justified. Assimilation of neighbor-

[1] *Jour. Geol.*, Vol. XXXIII (1925), pp. 509–25.

ing solid rocks is then appealed to as the process whereby this salic matter has been added, and with the aid of certain calculations some conclusions are reached as to the nature and quantity of the assimilated rock. In this last step of appealing to assimilation it is not necessary to follow Bain, for, as we shall see, there is an alternative that is perhaps to be preferred.

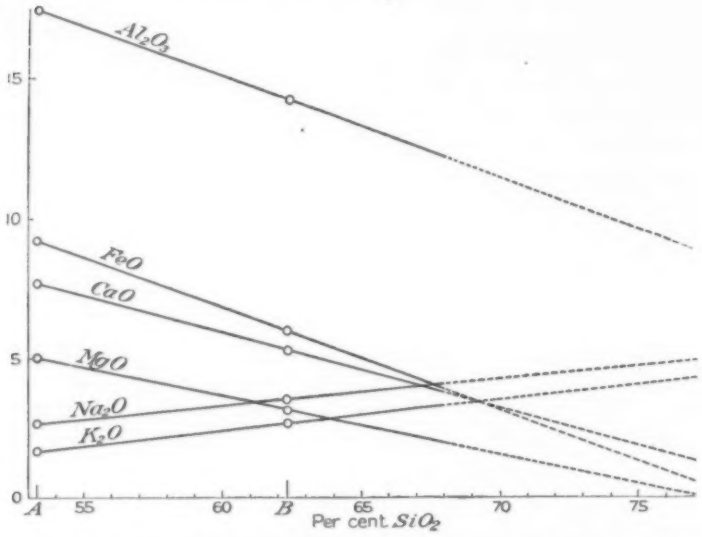

FIG. 1.—Diagrammatic presentation of the relation between the composition of *A*, the chilled phase representing the original composition of the magma and *B*, the average composition of the Sudbury sheet after Bain's determinations.

Accepting Bain's conclusion that something must have been added to the norite-micropegmatite mass as exposed in the Sudbury sections, we may seek a conclusion as to the nature of the added material in, perhaps, the simplest way with the aid of a straight-line or proportionality diagram such as Figure 1. On this diagram have been plotted at *A* the composition of the original magma as determined from the chilled base, and at *B* the average composition of the mass as computed by Bain. By joining the two points corresponding to each oxide and producing the lines thus determined

we obtain a graph of the possible compositions of material that could be added to *A* in order to change it to *B*. We note that the highest possible silica content of the added material is about 77 per cent since any more siliceous material would require to have a negative content of MgO. The Mississagi quartzite, with more than 89 per cent silica, is immediately ruled out. Appropriately diluted with iron formation and other rocks comparatively low in silica we could not, perhaps, summarily dismiss the possibility that the Mississagi quartzite thus modified was the material added, but since the magnesia content of the added matter would then necessarily be fairly large we must look to points on the diagram corresponding with at least 1 or 2 per cent MgO. This means a further marked decrease in the amount of silica, and when we consider compositions containing 70 per cent or less SiO_2 we have already reached a point where the amount of material required to be assimilated approaches the mass of the original magma itself, a fact which can be read directly from the straight-line proportions of the diagram (Fig. 1). Assimilation of such magnitude must assuredly give us pause.

When we turn to that portion of Figure 1 corresponding with less than 70 per cent SiO_2 another fact besides the great amount of assimilation that would be required is forcibly brought to our attention. This fact is the remarkable similarity of the less siliceous portion of the diagram with an ordinary variation diagram of a normal calci-alkalic series of igneous rocks. All of the curves have relative positions and slopes consonant with this interpretation, for the curves of such diagrams are nearly straight lines in compositions extending from about 53–68 per cent silica. We are thus forced to consider the great probability that *B* represents the average of a perfectly normal uncontaminated igneous series and that what has been added to *A* in order to produce *B* is not some extraneous material but a siliceous member or members of precisely the same differentiation series, say compositions corresponding with 68–69 per cent SiO_2 on our variation diagram as we may now call it. In other words the conclusion is that the exposed Sudbury sections of the norite sheet unduly emphasize the acid differentiates, unduly in the sense that there is a greater bulk of acid differentiates than could be produced by simple gravitative differentiation of the original norite (chilled

phase) having regard for the moderate amount of differentiates more basic than this original.

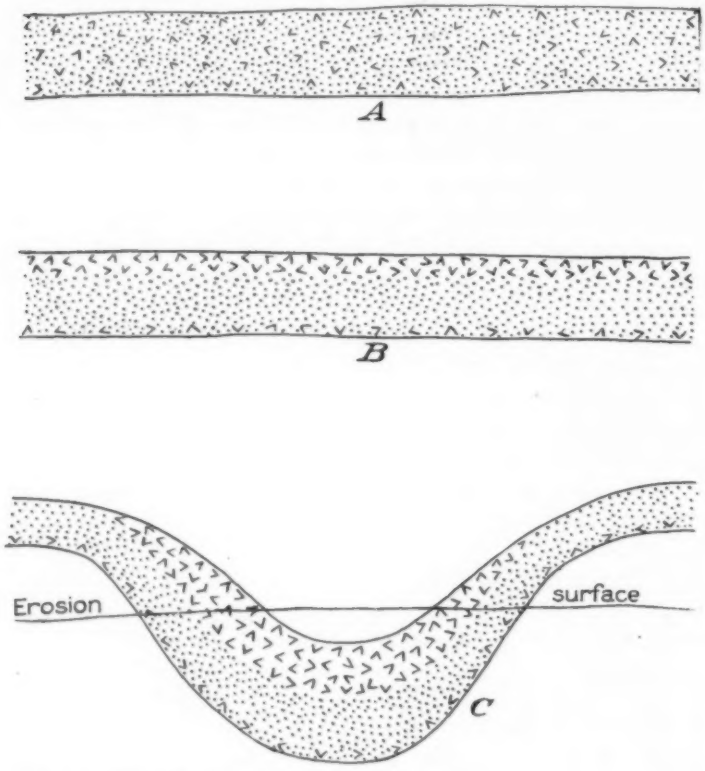

FIG. 2.—The Sudbury sheet:
A as intruded, homogeneous magma.
B when gravitative crystallization differentiation was well advanced.
C as deformed into a basin-shaped mass when the stage represented by *B* was reached.

This undue emphasis upon the acid differentiates might perhaps be referred to increase of linear outcrop of the acid portion by faulting or folding subsequent to consolidation, but it is more probably the result of a folding movement taking place during the differentia-

tion of the sheet, possibly, indeed, concomitant with the formation of the Sudbury basin. According to this interpretation orogenic movements occurred at a time when the lower noritic portions of the sheet were largely, if not entirely, crystalline and the upper more salic portions still liquid, and caused the inflow into the Sudbury section of a large amount of acid differentiate in addition to that formed there by the local action of gravitative crystallization-differentiation. In Figure 2 I have attempted to present this action diagrammatically.

Harker has suggested that the salic portion of the Sudbury mass represents a distinct separate intrusion of micropegmatite injected between the norite and overlying rocks.[1] While the explanation of the Sudbury relations here offered turns toward Harker's suggestion in some measure, it is really essentially different. The occurrence above the chilled basal phase of a few hundred feet of more basic rocks negatives the simple action proposed by Harker, for it necessitates the presence of an appropriate amount of complementary salic differentiate. In addition to this a much larger amount must be assumed to have moved in laterally, but it is to be regarded as precisely the same sort of differentiate generated simultaneously in adjacent parts of the same sheet.

We thus find that it is possible and perhaps preferable to regard the Sudbury mass as formed by direct differentiation without significant contamination of the magma by foreign matter of any kind.

October, 1925

[1] *Jour. Geol.*, Vol. XXIV (1916), p. 555.

REVIEWS

Traité de Géographie Physique (4th ed., entirely reset). Vol. I (*General Principles, Climate, Hydrography*). By EMM. DE MARTONNE. Paris: Armand Colin, 1925. Pp. xii+496, 193 text figs. and text maps, 6 plates, and 2 plates in color.

The complete rewriting for a new edition of a work of such value as the Martonne's *Treatise on Physical Geography* is an event of importance to geographers generally throughout the world. It is now nearly twenty years since the appearance of the first edition, at which time the work was crowned by the French Academy and by the Geographical Society of Paris. The work, expanded somewhat in the second and third editions, was already somewhat bulky for convenient use, and the new edition is to comprise three volumes of moderate size. The first of these, which has already appeared, will be especially welcome, since the 227 pages devoted to climate constitute the only recent original treatment of this subject by a French author, the last edition of Angot having appeared in 1916. The subject has been well handled and brought up to date.

The second volume is now finished, and it is hoped that it will issue from the press during the present year. This volume is given over entirely to the subject of earth relief. The preparation of the concluding volume, which will be devoted to biogeography, is being prepared in collaboration with two well-known French savants: the agronomist, M. A. Chevalier, director of the Colonial Laboratory of Agronomy of the French École des Hautes Études, and M. Cuénot, professor of zoölogy at the University of Nancy.

WILLIAM HERBERT HOBBS

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